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THE IDEAL OPERATION FOR ANEURISMS OF THE EXTREMITY.

REPORT OF A CASE.[1]

By Bertram M. Bernheim, M. D.,

Instructor in Clinical Surgery, The Johns Hopkins University, Baltimore, Md.

Toward the end of August, 1915, Mr. W. D., aged 43, from Sydney, Nova Scotia, was referred to me by his physician, Dr. Wm. Bruce, for the relief of an aneurism of the right popliteal artery. A definite history of syphilis, practically untreated and of many years standing, was obtained and corroborated by a positive Wassermann reaction. The popliteal swelling had been present only one month, but indefinite pains of a so-called rheumatic character had been observed in the region of the knee-joint for about three months. Once the tumor had made its appearance, however, its growth had been rather rapid, and it had given rise to a considerable amount of pain, and, in addition, a disturbance in the return circulation, manifested by a gradually increasing œdema, had become apparent in the lower leg. A certain degree of varicosity was present in the veins below the knee, but the egg-sized, oblong, pulsating tumor back of the knee was obviously the prime factor in the impaired venous return.

It was hardly to be expected that an adequate collateral circulation could have developed, in view of the short duration of the aneurism, and especially in view of the fact that the dorsalis pedis and posterior tibial arteries of the foot pulsated normally. Most imperative, therefore, was a careful search to determine the state of the collateral circulation; for a definite knowledge of this feature was essential to the selection of an intelligent operative procedure.

At two independent sittings, therefore, the Moszkowicz test,[2] as given by Dr. Matas, of Tulane University, was carefully carried out, the result being almost totally negative. Occlusion of the popliteal artery proximal to the aneurism left a limb whose bloodless, cadaveric appearance gave mute evidence of what would occur if, in the treatment of the aneurism, some means were not found for preserving the normal arterial flow. Nor was this a case in which time could be spared for an

[1] Read before The Johns Hopkins Hospital Medical Society, October 18, 1915.

[2] Testing the Efficiency of the Collateral Circulation as a Preliminary to the Occlusion of the Great Surgical Arteries. Rudolph Matas: Jour. Am. Med. Assn., Oct. 24, 1914, Vol. LXIII.

attempt to develop a collateral circulation, since the aneurism was increasing in size somewhat rapidly, and the man was very anxious to return to his work.

On September 3, at the Union Protestant Infirmary, I exposed the aneurism, prepared to do any operation that might be indicated. A spindle-shaped tumor, as shown in Fig. 1, presented and was opened on its dorsal aspect, revealing only two openings, the entrance and exit (Fig. 2) of the popliteal artery, the two points being distant about an inch and a half from one another, and only the faintest sign of a groove being apparent between them. The popliteal vein was so densely adherent to the sac that it was impossible to separate it without taking part of the sac wall, which, of course, was done.

To have attempted a reconstructive Matas endoaneurismorrhaphy under the circumstances would have amounted to little more than courting disaster. Hence, keeping in mind the insufficient collateral circulation, without further ado I removed about 15 cm. of the internal saphenous vein from the affected leg at the knee and, after proper preparation, interpolated [2] about 12 cm. of it between the severed ends of the popliteal artery. Only the ends of the sac were cut away (Fig. 3), the remainder being left to be folded around the transplant as a partial reinforcement (Fig. 4). Carrel's end-to-end suture was used, and the distal end was united to the graft first, because the artery was most deeply situated at that point. It is worthy of note that the wall of the artery was thicker than normal, had numerous pin-head-sized dull-gray plaques in its intima and was so friable that it tore badly in its preparation for suture, rendering necessary the removal of an additional centimeter and a half from the distal end. The suturing itself was accomplished without great difficulty, and at its conclusion blood went through the graft in a normal manner, except for the presence of the marked dilatation so graphically illustrated in Fig. 4.[4] The dorsalis pedis and posterior pulses could be felt at once and remained normally palpable at all times. An uninterrupted convalescence ensued and the patient returned home not only able to walk well but also, as he himself volunteered, "able to get on his shoe with a sock on his foot," whereas, prior to operation, this had been impossible owing to the œdema. All pain and discomfort in the leg had disappeared and a curious preoperative "dead feeling" of the great toe had given way to a normal feeling. Pulsation could be felt all along the vein graft as well as in the arteries of the foot.

So far as I have been able to determine, no case similar to this [5] has been reported from this country, and hardly more than seven from elsewhere [6]—a rarity that is most discouraging to those who had hoped for real practical developments in modern vascular surgery.[7] Perhaps other cases will be dealt with in a similar way, as the intelligent study of vascular conditions in general and of aneurisms in particular becomes more widespread. Many limbs have been sacrificed because a definite knowledge of the state of the collateral circulation was wanting at the time of operation. Dr. Matas, whose work in the field of aneurisms has brought him such well-deserved renown, has emphasized this point in numerous papers; and the case just cited is an eloquent proof of the necessity for this determination prior to operative interference. Many difficulties will be encountered in carrying out the tests, and they are not without a certain amount of danger; hence, they will probably have to be modified. Nevertheless, be that as it may, the knowledge is essential and must be had at all costs.

DISCUSSION.

DR. HALSTED: Dr. Bernheim is to be greatly congratulated. The indications for the transplantation seem to have been clear, and the operation was a complete success. It is well termed the "ideal operation," when the indications for transplantation are so definite as they were in the case just reported by Dr. Bernheim.

We are indebted to Alexis Carrel for making such an operation possible. Prof. Lexer, the distinguished director of the surgical clinic of the University of Jena, is responsible for the term, and he was probably the first to transplant a blood vessel in the treatment of aneurism. Many surgical procedures have been called "ideal" and for their time have, perhaps, deserved the appellation. Most of them were, however, short-lived. Some surgeons, myself [8] in the number, have advocated excision of the aneurism under certain conditions. By Bramann and by Delbet excision was termed the "ideal operation." Lexer reported his first case at a meeting of the Deutsche Gesellschaft für Chirurgie eight or nine years ago. The operation was for an aneurism in the axilla, the result of an attempt by some surgeon to reduce an old dislocation of the shoulder-joint. The operation was successful so far as concerned the patency of the vessels; but the patient died in a few days of delirium tremens.

Four or five years later Lexer reported a second case, also successful. In this a long piece of the saphenous vein was transplanted into the defect caused by the excision of a popliteal aneurism. Lexer has performed his "ideal operation" in a third case, the details of which I cannot at this moment recall. In several other instances in the human subject the transplantation of a vein to replace an arterial defect has been undertaken, usually with unsuccessful result. The surgeon who attempts this operation

[2] The vein segment was "reversed" so that its valves *faced* the foot instead of the heart, as they normally do. This is a most important step in every venous transplant, since the arterial current will be forced to break down the valves, unless they are reversed.

[4] It is hardly necessary to add that this dilatation is really a "normal" occurrence in every instance in which a vein is subjected to arterial pressure. The vein wall is thinner and has less muscle tissue than the arterial wall, and it therefore gives. Later on it hypertrophies and undergoes a fibrous change, which enables it to withstand continued arterial pressure without difficulty.

[5] The first operation of this kind was performed in 1906, by Lexer, who was then in Jena. In a paper appearing in the Arch. f. klin. Chir., 1907, lxxxiii, 458, he spoke of the procedure as "The Ideal Operation for Aneurisms," and it has gone by this name ever since.

[6] Abalos, J. B.: (Fusiform popliteal aneurism; extirpation, circular suture; reestablishment of normal circulation.) Spitalul, Bucuresci, 1914, xxxiv, 183-191.

[7] Surgery of the Vascular System. B. M. Bernheim. J. B. Lippincott Co., Phila., 1913.

[8] Ligation of the first portion of the left subclavian artery and excision of a subclavio-axillary aneurism. Johns Hopkins Hospital Bulletin. 1892, Vol. III, p. 93.

FIG. 1.—The aneurism and the accompanying vein are exposed, showing dense adherence of the vein to the sac-wall. The small dark bulging area in the sac-wall was the site of a threatened rupture of the sac, evidently caused by manipulations during the application of the Moszkowicz test.

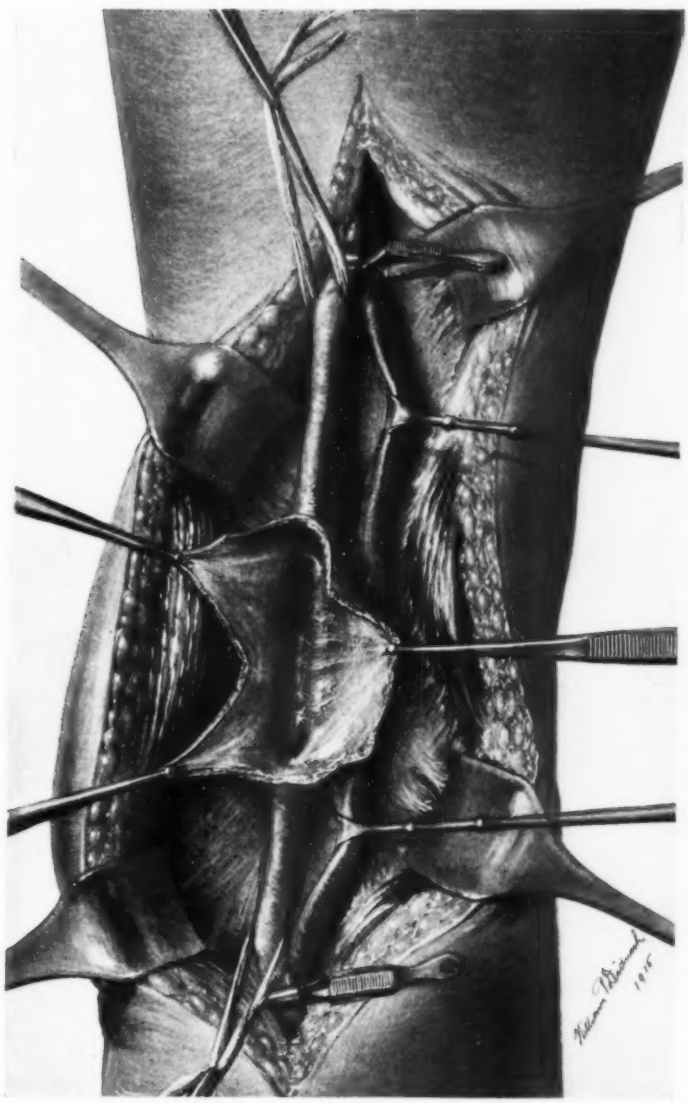

FIG. 2.—The aneurismal sac opened, showing the entrance and exit of the popliteal artery. The drawing greatly exaggerates the groove between these points.

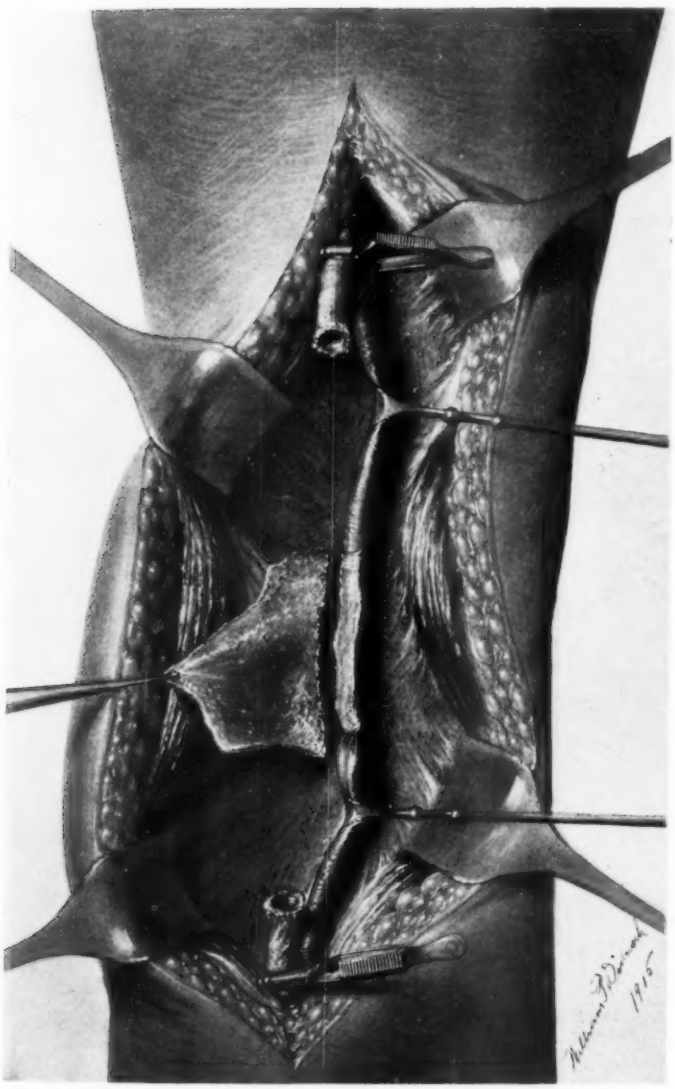

FIG. 3.—The aneurismal sac cut away from the popliteal artery. The vein was separated from the arterial sac by taking part of the sac-wall with it. With the exception of the ends of the sac, all the remaining portion was left *in situ*. The ends interfered with the placing of the venous graft.

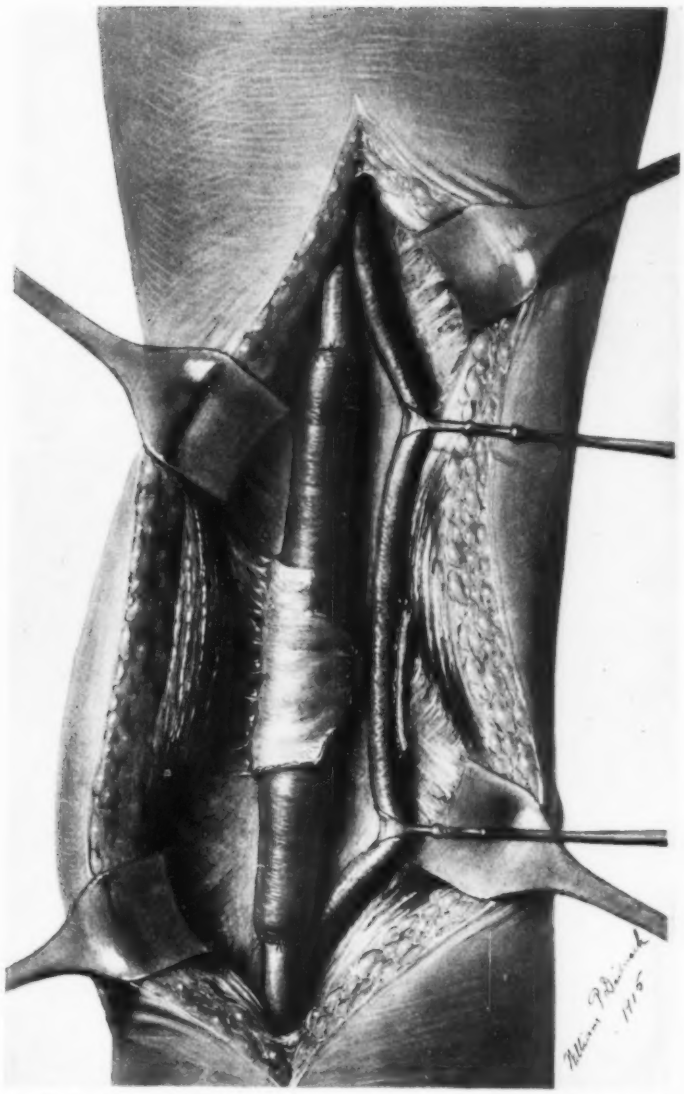

FIG. 4.—The vein graft has been sutured to the ends of the popliteal artery and the aneurismal sac wrapped around it for the purpose of reinforcement.

without having practised it on animals will almost surely fail to accomplish it successfully.

Six years ago I invited Dr. Bernheim to transplant for me a long piece (12 to 14 cm.) of the saphenous vein into an arterial defect caused by the excision of a sarcoma of the popliteal space. In this case the popliteal vein, the internal popliteal nerve and the popliteal artery, from Hunter's canal almost to its bifurcation into the tibials, had been excised. The vascular suture at the lower end of the space was quite difficult on account of the depth of the wound and the relatively small size of the distal stump of the artery. For a time the circulation through the transplant was perfect, but the interpolated vein became thrombosed[9] before the wound could be closed. Gangrene did not, however, ensue.

I might mention in this connection that an end-to-end suture of the aorta has been successfully accomplished. In excising a retroperitoneal tumor, Braun tore into the abdominal aorta and after excising about 2 cm. of this artery was able to sew the widely separated ends together.

Prof. Kümmel, of Hamburg, told me of a recent interesting experience of his own. On excising a tumor he made a hole in the abdominal aorta. This he closed with a suture of coarse silk and, if I remember correctly, without the use of oil or vaseline. A second uninterrupted suture of fine silk was taken to reinforce the first.

I should like to be the first to call attention to a possible flaw in my argument for practising the partial occlusion of an artery in the treatment of certain cases of aneurism. As some of you perhaps know, I advocate the employment of a band which can readily be removed and which does not injure the wall of the artery, in order to test and then to encourage the anastomotic circulation. But I realize that it may be possible, even with only partial occlusion, to interrupt the blood flow totally and too quickly. Thus, following the application of a band which still permits a small stream to flow through the artery to the aneurism, the latter might so promptly become solidified by the clotting of its contents that gangrene would be threatened. It is obvious that in such a case removal or loosening of the band might not restore the circulation through the aneurism.

My own experience with vascular suture in the human subject has extended only to veins and to the lateral suture of a defect of the femoral artery in Hunter's canal.

Of particular interest to me is the case of a patient upon whom, with the assistance of Dr. Heuer, I operated four or five years ago. We had about completed the removal of a very large lymphangiomatous cyst of the abdomen. There remained to be freed only its connections with the inferior vena cava. While these were being separated with extreme caution, blood gushed from this vein. There proved to be a linear defect in the vena cava so long that six artery clamps were required to close it. A lateral suture of the vessel with oiled, fine silk was successfully accomplished. The patient's convalescence was uneventful and she is at the present time in excellent health. The defect in the wall of the vein was not an artefact. It represented, I believe, an imperfectly closed orifice from the vein to a lymph-bud or lymphatic vessel from which the cyst had had its origin. I expect to report, later, this case and an analogous one, in detail, because they may serve to account for the occasional presence, hitherto unexplained, of blood in certain lymphangiomatous cysts, and for the observations that cysts which on the first tapping yielded a clear fluid have, on subsequent tappings, been found to contain more or less blood.

Thus Professor Jordan and Professor Voelcker,[10] of Heidelberg, refer to a case of cyst of the neck, reported by Weil, which had its origin, he believed, in a hemorrhage from the vascular wall of a cystic lymphangioma. In support of Weil's view, the authors instance the observations, repeatedly made, that cysts from which at the first puncture only a clear serous fluid was withdrawn at subsequent aspirations sometimes yielded blood. And one frequently meets with the statement that the serous content of lymphangiomatous cysts may *after injury* become bloody.

As a possible explanation of the occasional presence of blood in lymphangiomatous cysts I would suggest that a primordial communication between the vein and the lymphatic cyst may not have been completely closed. The presence of blood, at subsequent aspirations, in the content of cysts, which at the first tapping had yielded only a clear serum, might be due to the relief of tension in the cyst rather than to an injury of its wall; for the pressure within the cyst being diminished or negatived, there might be a retrograde flow of blood from the original venous connection.

In one of my cases, a supraclavicular hygroma, a definite relation to a large vein of the neck was demonstrated, and in the other, as I have related, there was an intimate connection with the inferior vena cava and evidence of an opening between the cyst and the vein which may have become closed more or less completely by a cribriform fascia of some sort. It is, I believe, very improbable that the thin non-vascular walls of either of these lymphangiomatous cysts could have contributed much, if indeed any, blood to their contents; and to continuously furnish blood enough to stain the fluid for possible frequent subsequent tappings would, it seems to me, have been, for such walls, impossible.

DR. BERNHEIM: It had not occurred to me to do the Moskowicz test on the other leg, as suggested by Dr. Halsted. We had trouble enough doing it on the one. It seemed quite painful and, to tell the truth, I thought it not devoid of danger. We used a small roll of gauze about 2 inches wide, one end of which was made into the shape of a cone, so as to facilitate the making of pressure between the big muscles just above the knee. With this in the palm of my hand, I clamped down on the artery above the aneurism, but the vein must have been shut off at the same time. I realize that an instrument has certain advantages over the hand in making this pressure, but it also has disadvantages that, to my mind, outweigh the advantages. Any one who has attempted to get the popliteal pulse, and has had the usual difficulty in locating and palpating it accurately, will realize the amount of force necessary to occlude it. This in itself is, perhaps, not so terrible, but one can never be sure that an offshoot of the aneurism is not caught under the pad. Hence I have about decided not to use the test in that form any more. It would seem far more conclusive to make an incision above the aneurism and apply a band which will occlude the artery entirely—but only the artery. With a dressing in place over the small wound, the collateral circulation could be determined with positive accuracy and with great promptness; and, best of all, such a procedure would be entirely devoid of danger. At its conclusion the operative procedure indicated could be carried out with a sense of comfort and confidence that is not felt at present.

[9] The most serious objection to the "ideal" or veingrafting operation is perhaps this: that in case of failure the thrombosis which starts in the graft may extend either centrally or peripherally, or in both directions, from the interpolated vein into the artery and thus involve important anastomotic branches which would not have been threatened with occlusion if the artery had been merely ligated, or the sac merely excised or plicated. The transplanted vein is, consequently, a menace, for in at least two-thirds of the cases in which the "ideal operation" has been practiced, thrombosis has occurred in the insert.

[10] Handbuch der praktischen Chirurgie, Bd. II., S. 117.

MEMORIAL MEETING TO DR. E. L. TRUDEAU.*

THE LAENNEC.

DR. JANEWAY: Since the Laennec Society last met here a famous figure in the field of tuberculosis study has passed away—Dr. E. L. Trudeau. Not only was Dr. Trudeau the most famous pioneer of tuberculosis investigation and treatment in America, but he was in a very particular way the hero of all of us physicians in this country throughout a long life.

In the year in which I was born, Dr. Trudeau, then looking forward to a highly promising career in medicine in New York City, came into my father's office and was told by him that he had very definite signs of tuberculosis of the lungs, and his career seemed, in all human probability, a failure. How different he made it! From that time on, whenever I would become discouraged with the way things were going with me, my father would take occasion to point out to me what Dr. Trudeau had accomplished from an apparently hopeless outlook. Nothing was much more hopeless at that period than the outlook for a physician with tuberculosis; but his career has been one of the most brilliant in the history of American medicine, and one of the most fruitful, both in the quality of his scientific achievement and in what he accomplished for sick men and women.

Dr. Trudeau was not himself a Hopkins man, indeed he could not have been, for the Hopkins Medical School did not exist at that time, but he sent his two sons here; and while we are all hero-worshippers (everyone worth his salt is, and especially every physician) I have come to believe that Hopkins men are particularly hero-worshippers of the right sort of medical heroes. So it seems especially fitting that this first meeting of the Laennec Society for the year should be given over to an attempt to bring before those of us who had the privilege of knowing Dr. Trudeau, and to many of you who only knew him by reputation, the achievements, and still more, the characteristics which marked his career and which are such a stimulating example to every one of us.

We had hoped that Dr. Welch might be here and speak from the fullness of his personal knowledge and appreciation of Dr. Trudeau's scientific work, but a meeting of the China Medical Board, which it was imperative he should attend, was called for to-day. We are, however, exceedingly fortunate in having with us three men who knew Dr. Trudeau intimately; one his senior pupil, as it were, and all three his personal friends, who will speak to us on the different aspects of Dr. Trudeau's life and character, so far as it is possible to separate the different aspects from one another. That any of them can think of Dr. Trudeau other than as the man, is impossible, but each will try to tell us something of that side of Dr. Trudeau which is set opposite his name on this evening's program. Of course, if Dr. Osler were here, he would have spoken of Dr. Trudeau with that enthusiasm and insight which we all know how to expect from him. Though absent, he still is with us to-night in spirit, and has sent us a cablegram which I will read to you:

Through failure to success, Trudeau passes among the elect. Human sympathy and unfailing optimism made him a strong defense to the stricken. The noble example of his life will remain a permanent inspiration in our profession.

OSLER.

Dr. Walter B. James, of New York, will speak to us of Dr. Trudeau, the physician.

DR. TRUDEAU, THE PHYSICIAN.

BY WALTER B. JAMES.

It is fitting that this memorial meeting should be held in Baltimore, for Dr. Trudeau cherished a warm affection for The Johns Hopkins, and turned to it for help in some of his periods of greatest suffering, and he knew that nowhere was he more sincerely loved than here, and nowhere was his work more appreciated. It is fitting, too, that it should be at a meeting of the Laennec Society, for Trudeau and Laennec had much in common.

They both possessed the Gallic temperament and the power to be cheerful under overwhelming vicissitudes, and so, like their nation to-day, snatch victory from defeat. Both were subjects of the very disease that was their life study, and to which they made numerous and valuable contributions, and each, a devout Christian, awaited, with fortitude and without complaint, the coming of the end whose signs he knew only too well.

The brief and impressive document to which each member of our profession must assent before he may enter upon its practice, in spite of its antiquity and its archaic phrases, is still the best epitome of the duties of the true physician; and the Hippocratic oath remains the standard for the conduct of the medical life. To treat the sick with no thought for himself, but with every thought for them, and with honesty and truth to teach to those who follow him all the knowledge he has acquired.

Had the vague yearnings of mankind to know the nature and origin of the maladies that plague them at that time crystallized into the activities that we know now as medical research, this, too, would doubtless have been included in the oath.

This document breathes in its every phrase the highest spirit of medicine and, tested by it, Dr. Trudeau's life stands out as a splendid model for the physician.

It is 36 years ago this coming summer that I first heard of him. I had gone to the Adirondacks, to Blue Mountain Lake, to recuperate from the effects of a winter of too great enthusiasm for biology under the stimulating influence of your Newell Martin.

The Adirondacks was a wild region compared to what it is at present, but rumors reached me of a young doctor whose fame was widespread in the northern district, 70 or 80 miles away. I heard nothing of his professional renown but only that he was a splendid hunter and a remarkable shot with the rifle.

Laennec, too, was a sportsman, and it is related of him that his friends accused him of taking more pride in his horsemanship than in his professional achievements.

* Proceedings of the Laennec Society, January 24, 1916.

A few years later it was generally admitted by us all in the laboratories of New York and elsewhere, that the only man from whom a trustworthy and pure culture of tubercle bacillus could be obtained was Dr. Trudeau. Then we heard that he had built a small sanatorium on a novel plan; soon, that he was one of the best men in the country to whom to send cases of early tuberculosis.

Not long after, I met him for the first time at Paul Smith's, when on a summer trip through the woods, and I was at once impressed by his tall, straight, alert frame, his keen and responsive interest and the sweetness of his smile. By that time he had begun to be famous throughout the world as a physician. This was the beginning of one of the most cherished friendships of my life.

The exigencies of this evening's program separate the achievements of the physician from those of the investigator and the man, but I suspect that in few men have these three activities been more closely tied together. His professional life was spent in a little village in the woods where distractions and amusements were few and of the simplest and most natural kind. His real friends were his patients, and they paid him occasional visits in the morning for professional advice, then came to spend evening after evening in his cheery library throughout the long winters, a little company closely knit together by the ties of isolation and common misfortune, but a company, than which one will rarely find one more light-hearted and gay.

His experimental laboratory, too, at first in his house, was later a structure so closely adjacent to it that the visitor, from his bedroom window, looked across a few feet of deep snow to the sturdy stone building where the experiments on immunity and resistance were being carried on.

There were no fixed hours for work or for rest, and even the examining office and dispensary of the sanatorium were in his own home. Thus his practice, his hospital and his laboratory were part of his daily life, and inseparable from it, and he never bore well a longer absence from them than one or two weeks.

I first heard from him a quotation which, we used to agree, briefly but comprehensively described the ideal aim of a physician. "Guérir quelque fois, soulager souvent, consoler toujours," and this describes not only his aim but what he made his own life express.

He would have been a distinguished and successful physician under any circumstances and in any circle, for he had the fundamental intellectual qualities that make for success in our profession. He had unusual diagnostic acumen, for he had the faculty of brushing aside or ignoring the little things that so often lead the mind of the unwary clinician from the straight road to truth, and, with an instinct that often seemed like a woman's, he would arrive at a correct diagnosis by means that were not easy to follow.

A skeptical stranger once presented himself to him for examination and the doctor found the chest painted with iodine over the right lung apex. After careful search he discovered signs of very slight phthisis in the left apex. He then asked the patient why he had painted the sound side; "To see if you would know" was the reply—a device that might well have tripped a less experienced and wary examiner. He told me this tale with keen enjoyment, for his sense of humor was exceptionally well developed, and stood him in good stead on many occasions of sore trial. A sense of humor seems a necessary part of the equipment of the complete physician.

When we think of how his life was passed in the woods, surrounded by cases of one single disease, his attention focused upon this with singular intensity, for he engaged in but little general practice, it is remarkable that he so rarely erred, and confounded other conditions with tuberculosis. He was especially happy and successful in his relation to that always puzzling group of cases, where lassitude and slight loss of weight with perhaps rapidity of the pulse are the only symptoms that suggest in young people possible tubercular trouble in the lungs; and he rarely erred.

His invaluable contributions to the rest and fresh air treatment of tuberculosis I shall not describe, for these will be spoken of to-night by one whose relations to Dr. Trudeau's work were even closer than mine, but I should like to say a word about one phase of his therapeutic achievements that belongs to his life as a practicing physician.

Every doctor is conscious at times of a feeling of disappointment, even of resentment, when advice which is founded upon the principles of modern scientific medicine is unheeded by the patient, and we are generally human enough to ascribe it to the vagaries of the individual, rather than to our own delinquencies.

When one has viewed the work of one like Dr. Trudeau from close at hand as I have done, and has marveled at his ability to secure the conscientious performance of every therapeutic duty prescribed, exposure hour after hour to cold, the tedium of months spent at absolute rest in bed, with a prescribed diet, one wonders what quality it could be that the man possessed enabling him to so override objections and secure the cooperation of his patients. Was it not that indefinable quality which some men have of carrying conviction by simple words, of impelling belief in the wisdom of counsel, of inspiring confidence through singleness of devotion to their patients' interest? A quality of inestimable value to a doctor.

The duty of the physician does not stop with the writing of a wise prescription or the giving of advice, and it is the one who can secure the carrying out of his counsels who fills the full measure of his responsibilities. Many of Trudeau's clinical victories resulted from this ability to secure the complete out-of-door rest cure for the patients who had refused such measures until they fell into his hands.

His unworldliness and his forgetfulness of self were nowhere more in evidence than in his relation to the very poor, who soon flocked to Saranac Lake with a hopefulness that was pathetic.

Trudeau was a born teacher. He had the spirit of Erasmus. It was not many months after the establishment of his little home-made laboratory in his house at Saranac Lake before men began coming to him to learn how to culture the tubercle

bacillus, how to isolate it and, above all, how to design sanatoria and carry out the principles of out-door treatment.

I can remember that during the years when he was enjoying a fair measure of physical vigor, and was able to come to New York once or twice in the winter for 10 days or two weeks at a time, it was always understood between us that he should give one of my clinics at the college, or possibly a lecture, and during those years I made it a point that every one of my classes should have the opportunity of hearing his views on the early diagnosis and treatment of tuberculosis; and he was never happier than on these occasions.

In his autobiography he tells how a doctor brought, all the way from Australia to Saranac Lake, a young man with tuberculosis, because he himself had heard Trudeau describe the out-door cure and its result on one of the above occasions. Later his voice became so thin that he could be heard only with difficulty in the lecture room, and, to my sorrow and his, he was reluctantly compelled to give up this annual function.

The Hippocratic injunction to teach was one that he obeyed throughout his entire working life, and Saranac Lake became a Mecca for those in search of information regarding tuberculosis. The sanatorium that he founded and in which he took so much pride soon became a model for such institutions.

The most striking and stimulating phase of his teaching activity was his influence upon young physicians who came to him disabled by the disease. It was an occurrence pathetically frequent for medical students or recent graduates to come to me with the familiar story of a slight hemorrhage, or afternoon fever, or a few tubercle bacilli found in the sputum, always downcast and discouraged, and with a feeling that their professional career was at an end. Generally I had little difficulty in cheering them up when I gave them an account of Trudeau, what he had accomplished and what other men in their situation had succeeded in doing in spite of these obstacles; and I would send them to Saranac with a letter to him and with advice to put themselves entirely in his hands, for I knew what the result would be; a few weeks or a few months, perhaps even a year, spent on an open porch, later with an electric light over the couch where, presently, medical literature, especially the literature of tuberculosis, would be studied; then, with the absence of fever and the return, in some measure, of physical vigor, an opportunity to do a few hours' or a half day's work in the laboratory, and so the establishment of an interest that would last for the rest of their lives and leave them further advanced in their profession at the end of their cure than they would have been had they pursued their original course of life.

Many of the most useful and productive workers in the field of tuberculosis, now scattered throughout this country and Canada, are "graduates of Saranac," who there, under Trudeau, found health and at the same time opportunity and encouragement to enter upon a life of research.

I have never known Dr. Trudeau's front door in Saranac Lake to be locked, and every evening for many, many years in his cosy library there was an informal gathering of the younger and older doctors, who sat at his feet, and the discussion was almost always of matters in some way related to the disease that had brought them there. These gatherings, in which I have from time to time enjoyed the privilege of taking part, constituted a school in the truest sense, and a school that reminds one of what we read in the history of the early days of learning.

His patience in listening to his pupils, his kindliness and complete absence of arrogance, the freedom with which he gave to everyone all that he knew—these qualities, together with the indefinable charm that drew these young men to him, made him a great teacher. Just as an interest in the study of tuberculosis had been of inestimable value to him in stimulating his own recovery, so I am sure did he lead many a young doctor back to health, through the development of a similar interest.

It is a fundamental law of nature that effort is stimulated by resistance, and in some of the recent results of the comparatively new science of anthropogeography, I am often reminded of Trudeau and what I believe his environment did for him. As Huntington shows that in factories the workers put forth their maximum production in periods of greatest climatic rigor, and when he shows that the degree of civilization in Europe now and in the past coincides with areas of maximum climatic storminess, it tempts one to the stimulating and encouraging thought, that perhaps the storminess of Trudeau's fate acting upon his strong nature helped to produce the unusual result that we are celebrating to-night.

As the direct result of his splendid medical life he has left behind him in Saranac Lake a group of institutions that will endure—a sanatorium that is a model for all the world, teaching how cases of early tuberculosis should be managed, with productive research laboratories maintaining high ideals of work and especially utilizing the skill and intelligence of physicians while they are completing their recovery.

But, best of all, he has left a spirit which animates all medical life that is worth while, and which is the soul of modern medical science; research for the sake of practice, and practice for the sake of humanity.

The value of such a life as Trudeau's to our profession cannot be estimated. For many years an influence has radiated from the village in the woods stimulating men to a fuller and more perfect carrying out of its fundamental precepts. But as matter is indestructible and as force is indestructible, so the power of such a life as his is imperishable. It will go on year after year, helping generation after generation of men to practice medicine better because he lived so complete and so perfect a medical life, even though in so remote a spot.

> "So be my passing!
> My task accomplished and the long day done,
> My wages taken, and in my heart some late lark singing,
> Let me be gathered to the quiet West,
> The sundown splendid and serene."

DR. JANEWAY: Dr. James has told us something of the school which gradually grew up around Dr. Trudeau at Saranac Lake, one of the few really distinctive American schools in medicine.

There has come to us to-night from Saranac Lake the oldest pupil of that school, a pupil upon whom the headship must

fall, now that Dr. Trudeau has gone; the man to whom we in this country look for the most authoritative opinion both upon the clinical problems of tuberculosis and those complex and still unsolved problems of immunity to it. Dr. Baldwin, of Saranac Lake, will speak to us on Dr. Trudeau, the investigator.

DR. TRUDEAU, THE INVESTIGATOR.

BY DR. E. R. BALDWIN.

It is 15 years since Dr. Trudeau himself gave a reminiscent talk before this society on the history of his experimental work. It is most gracious of your president to grant me the privilege of reviving the memory of that account. It was a story of an extraordinary effort by an extraordinary man. Those now present who listened to him will readily recall his dramatic description of the home-made incubator box. He always spoke humorously of the bacilli freezing and thawing, but was very proud of his achievement in growing the tubercle bacillus so early with his primitive apparatus. He was, in truth, a pioneer in a field little touched by those not directly concerned with tuberculosis. By that I mean those who never had it themselves. No description that I can give will compare with his earnest, entertaining style in relating his early difficulties and experiments. I cannot refrain from quoting his own words found in his autobiography just published (Chapter XVI, pp. 201-203):

In the fall of 1885, as soon as I had equipped my little laboratory room, I began to work. At first my knowledge was limited to the detection of the tubercle bacillus in the secretions of patients, and my observations to verifying Koch's claim that this bacillus was the cause of the disease and was always found when tuberculosis was present. I made examinations of all my cases, and as a result found only one patient in whom, while the symptoms of consumption of the lungs were present, I could never detect the bacillus. I made a study of this case and proved that it could not be tuberculosis, as the expectoration would not kill animals, while the expectorated matter which contained tubercle bacilli always produced generalized tuberculosis in the guinea-pigs. I published this study under the title of "An Experimental Research upon the Infectiousness of Non-bacillary Phthisis," in the *American Journal of the Medical Sciences* for October, 1885, and this was my first publication from my little laboratory room. I am afraid I have been guilty of many others since!

The thing I craved to do, however, was to succeed in cultivating the tubercle bacillus outside of the body and then produce the disease with it in animals. It was the early winter of 1885 when I attacked this problem with great earnestness. I had learned from Dr. Prudden how to make artificial media—beef gelatin, beef agar and other media—but the first growth of the tubercle bacillus direct from animal tissue I knew could be obtained only on solidified blood serum, and then with difficulty. I bought a small sheep for three dollars and a half, and from the sacrifice of this animal I procured the required amount of blood, which, thanks to the pure air and the snow upon the ground, remained tolerably free from contamination and was transferred at once to the ice-box to coagulate. I am afraid my associates at the laboratory to-day would hardly consider the technique I then employed up-to-date, but after many incidents I succeeded in getting some fair plants of blood serum in tubes.

I made plants on this blood serum from a tuberculous gland removed from one of my inoculated guinea-pigs, and put all the tubes in my home-made thermostat. For the next two weeks I watched the temperature of my absurd little oven with jealous care, and I remember one very cold night getting up in the night and going down stairs to look at the temperature. Many of the tubes turned out at once to be contaminated and a variety of growths appeared on them; but after ten days I still had four tubes free from contamination, and these looked much as when I first put them in the incubator. On the eighteenth day I thought I detected a little growth in the corner of one of these. With every precaution against contamination, with my platinum spade I removed a little of the suspected growth and rubbed it on a couple of clean slides, dried it and stained it. My first intimation of success was when one or two large masses on the slide refused to decolorize when treated with the acid. I washed the slide, put it under the microscope, and to my intense joy I saw nothing but well-stained culture masses and a few detached tubercle bacilli. I at once planted some fresh tubes from the one I had examined, and I knew now I had pure cultures to work with. This little scum on the serum was consumption in a tangible form. With it I could inoculate animals and try experiments to destroy the germ. (An Autobiography, Dr. E. L. Trudeau, pp. 201-203.)

After mastering the culture of the bacillus he naturally made efforts to show the actual effect of therapeutic agents on it. This perfectly obvious and rational procedure, as a prerequisite to the employment of supposed germicidal treatment, indicates that the 10 years of wilderness life had not dulled his intellectual gifts. To quote his own words (Autobiography, p. 204):

As soon as I had pure cultures I began to inoculate rabbits and guinea-pigs, and started some experiments to try to kill the germ in their tissues by the injection of various germicides, such as creosote, carbolic acid, and other substances known to destroy germs. These experiments of mine all failed, and I found, as I expressed it to the students one day at the College, that "the tubercle bacillus bore cheerfully a degree of medication which proved fatal to its host!"

He also tried hydrogen sulphide and hydrofluoric acid gas, the latter forming the subject of his third contribution in print. Later (1888), he published clinical experiments with hot-air inhalations, the conclusions from which were so logical and well-stated as to give an index of his good reasoning faculties and clearness of expression. His summary is as follows:

First, the therapeutic value of hot-air inhalations in phthisis is doubtful. Second, the evidence obtained by the bacteriological study of the cases does not confirm the assumption that inhalations of heated air can either prevent the growth of the tubercle bacillus in the lungs of living individuals or diminish the virulence of this microbe when it has gained access to them. (Trans. Assn. Am. Phys., 1889, Vol. IV, p. 291.)

The work that probably attracted most attention to Dr. Trudeau as a scientist was the so-called Environment Experiment which he did in 1886 and repeated in 1887. I refer to the really simple but brilliantly executed experiment whereby he demonstrated on rabbits three very important truths.

The first one was that confinement, bad air, and restricted food without the bacillus could not produce tuberculosis. The second, that the conditions just described plus inoculation with tubercle bacilli led as a rule to a fatal infection. Finally, rabbits similarly inoculated and turned loose on a little island near his summer camp, recovered; in fact, he had to shoot them to catch them! It must have been a great encouragement to him, and it has rarely happened that a therapeutic

experiment has received such a widespread demonstration of its value during the lifetime of the author.

Here is his own description of the confidence it gave him:

This showed me conclusively that bad surroundings of themselves could not produce tuberculosis, and when once the germs had gained access to the body, the course of the disease was greatly influenced by a favorable or an unfavorable environment. The essence of sanatorium treatment was a favorable environment so far as climate, fresh air, food, and the regulation of the patient's habits were concerned, and I felt greatly encouraged as to the soundness of the method of treatment the sanitarium represented, even though it did not aim directly at the destruction of the germ. (pp. 204-206.)

The simplicity of this experiment and the novelty of its application made a strong appeal to his friends both lay and medical. I well remember the story of this experiment related to me when a medical student.

During each of the succeeding years until 1896 Dr. Trudeau was able to present some reports of experimental work from his laboratory. The most interesting of his contributions, and the most important, were his experiments with tuberculin. Influenced by the published work of Pasteur on protective inoculations he had engaged in much the same line of experiments simultaneously with Koch. His results were unfavorable and were published on November 22, 1890, shortly after the premature announcement of tuberculin as a cure by Koch in August of the same year. Dr. Trudeau obtained, nevertheless, prolongation of life in his treated guinea-pigs, and from that time until his death held to his faith in the possibilities of tuberculin and other specific vaccines. It must be conceded that this buoyant hopefulness was succeeded by skepticism in his periods of depression, but it was always founded on faith in his observations on animals. Before the discovery of tuberculin I found a short note of his in the Transactions of the Association of American Physicians, in May, 1890, when, in discussing one of the papers on tuberculosis, he indicated his faith in Pasteur's work. He said:

I think, perhaps, Dr. Shakespeare has not presented the most encouraging side of Pasteur's work. Pasteur has not only taught us that anthrax and chicken cholera are due to a germ, and that hydrophobia is caused by a specific virus which has its seat in the spinal cord, but he has taught us as well that by his methods these diseases are now already somewhat under control of man, and can be prevented to a great extent. We need not, therefore, necessarily take too gloomy a view so far as tuberculosis is concerned.

This faith was maintained throughout the quarter-century of life that remained to him. As evidence I will read a letter written last summer to the *British Journal of Tuberculosis* immediately before his fatal illness:

Nothing has occurred to diminish my faith in the value of tuberculin treatment—a faith which has been manifested by my continuing its use uninterruptedly in my practice and at the Adirondack Cottage Sanitarium ever since it was discovered, and through all the long years I stood nearly alone in my medical environment in its advocacy. If skilfully used, tuberculin stimulates the defensive resources of the organism and is a valuable adjunct to our treatment in many cases. I see no reason why continued research should not in time give us a better sensitizer than tuberculin.

During the years 1892 to 1895 various tuberculins were introduced that had supposed advantages. Dr. Trudeau expressed the hope that some improvements would be made by which the toxic effects could be eliminated. It was during the period that I began to work with him. Our hopes for the discovery of an efficient vaccine or antitoxin were high at that time. His own time was more than ever occupied with the sanitarium, and his laboratory work was desultory; yet during the winters (the only time he was able to work at all) he continued to direct experiments of the greatest variety. I find on our records over 50 different attempts to immunize animals with dead and living bacilli of varying virulence, and with varying dosage, intervals, etc. These he himself instituted during the years 1892 to 1900, besides numerous other experiments in which he was less directly concerned. At the opening of the Phipps Institute at Philadelphia, in 1903, Dr. Trudeau referred to his successful demonstrations of the immunity reaction obtainable in rabbits as follows:

Most of my own work has been devoted to the study of methods which might tend to produce artificial immunity . . . . It was only when I began to make use of living cultures as a protective inoculation that I met with any encouraging results, and my experience would indicate that the living germ is essential to what success has been attained in the production of artificial immunity against tuberculosis.

By preventive inoculations of living-bird tubercle bacilli in rabbits, I got undoubted evidence of a marked degree of artificial immunity in experiments which I reported to the Association of American Physicians in May, 1893. I was able then to demonstrate to the association, by means of living animals, that in rabbits having previously received the preventive injections of living-bird bacilli, the virulent inoculation at first gave rise to a violent reaction of the tissues, which ended generally in cure, while the tuberculous process similarly induced in the controls was accompanied by little or no local reaction. I have many times since confirmed these results by various experiments. (The History of the Tuberculosis Work at Saranac Lake. Med. News, October 24, 1903, p. 8.)

Altogether Dr. Trudeau published but few experimental studies,[1] his periods of ill-health, the sad deaths of his children, and many burdens incident to the sanitarium, made it impossible for him to do more than oversee some of the experiments in which he was most interested. One of his greatest disappointments was the fact that he could not work in the new laboratory generously built for him by Mr. George Cooper in 1894. He often exclaimed that he desired nothing better than a chance to work there and took great pride in showing visitors through.

Only those who knew Dr. Trudeau at home and in his laboratory during his prime could appreciate his enthusiasm for scientific investigation. Those who did not know him in the surroundings that he created whereby he was able to attempt experimental work, quite unique in character for the time, cannot know what pride he had in it. His enthusiasm and pride, nevertheless, never led him to much conceit about his knowl-

[1] Eighteen in all.

edge; this was one virtue that made him so companionable. He was ever modest in assertion, in reality rather timid, even among the young men about him who were naturally deferential. His boasting was too often applied to his associates! A little achievement was always the object of praise from him, and those of us who happened upon some minor point of interest were very likely to hear it enlarged upon by some of our colleagues who had heard of it from Dr. Trudeau.

His manner was irresistibly entertaining to a group of visitors. They often acquired more interest in scientific medicine and were cured of antivivisection heresies by a little talk from Dr. Trudeau about the tubercle bacillus. "Here is a little devil," he would say, "that grows with equal facility in a prince or a pauper," as he exhibited a tube culture to a group of admiring listeners. "Here is what people come to Saranac to be cured of in three months," he would cheerfully remark, as he produced a specimen of extensively cavitated lungs of a rabbit!

While his conversation at such times was dramatic, it was never vain or boastful of his own accomplishments; more often it was of embarrassing predictions of what his assistants were to discover! I can well remember the first weeks in his first laboratory during the winter of 1892 and 1893. He was wont to say: "Baldwin, how does this strike you? If you don't think I'm right I wish you would say so." This was well adapted to increase my conceit in my little stock of knowledge, but it gave a rare impulse to independent thinking. I had never encountered such modesty in any of my teachers and do not think Dr. Trudeau inculcated humility in his associates by this method, though by example this was possible. His belief in research was a passion for its humanitarian service, and he believed in science and humanitarianism joined together. He looked forward to a brighter day when all pain should vanish as a consequence of that union.

In closing I think a favorite expression or aphorism that he frequently used will be of interest to repeat: "The sanitarium represents what we know now, the laboratory what we hope to know in the future."

ARTICLES WRITTEN BY DR. E. L. TRUDEAU, M. D., REPRESENTING HIS EXPERIMENTAL LABORATORY STUDIES.

1. An Experimental Research upon the Infectiousness of Non-Bacillary Phthisis. Amer. Jour. Med. Sci., October, 1885.
2. Environment in its Relation to the Progress of Bacterial Invasion in Tuberculosis. Amer. Jour. Med. Sci., July, 1887; Trans. Am. Climat. Assn., 1887, IV, 131.
3. An Environment Experiment Repeated. Trans. Am. Climat. Assn., 1888, V, 91.
4. Sulphuretted Hydrogen *versus* the Tubercle Bacillus. Med. News, 1887, LI, 570.
5. Hydrofluoric Acid as a Destructive Agent to the Tubercle Bacillus. Med. News, May 5, 1888, LII, 486.
6. Hot-Air Inhalations in Pulmonary Tuberculosis. Med. News, 1889, September 28; Trans. Assn. Am. Phys., 1889, p. 287.
7. Some Cultures of the Tubercle Bacillus, Illustrating Variations in the Mode of Growth and Pathogenic Properties. Trans. Assn. Am. Phys., 1890, V, 183.
8. An Experimental Study of Preventive Inoculation in Tuberculosis. Med. Record, 1890, November 22.
9. The Treatment of Experimental Tuberculosis by Koch's Tuberculin, Hunter's Modification, and other Products of the Tubercle Bacillus. Trans. Assn. Am. Phys., 1892, p. 87; Med. News, 1892, September 7.
10. Results of the Employment of Tuberculin and its Modifications at the Adirondack Cottage Sanitarium. Med. News, 1892, September 10.
11. Eye Tuberculosis and Anti-Tubercular Inoculation in the Rabbit. Trans. Assn. Am. Phys., 1893, p. 108; N. Y. Med. Jour., 1893, July 22.
12. A Report of the Ultimate Results Obtained in Experimental Eye Tuberculosis by Tuberculin Treatment and Anti-Tuberculous Inoculation. Trans. Assn. Am. Phys., 1894, p. 168; Med. News, 1894, September 29.
13. A Chemical and Experimental Research on "Anti-Phthisin" (Klebs). (In collaboration with E. R. Baldwin.) Med. Record, 1895, December 21.
14. The Tuberculin Test in Incipient and Suspected Pulmonary Tuberculosis. Med. News, 1897, May 29, p. 687.
15. The Need of an Improved Technic in the Manufacture of Koch's "TR" Tuberculin. Med. News, 1897, August 28, p. 257.
16. Experimental Studies on the Preparation and Effects of Antitoxins for Tuberculosis. (In collaboration with E. R. Baldwin.) Am. Jour. Med. Sci., December, 1898, and January, 1899; résumé in: Trans. Assn. Am. Phys., 1898, p. 111.
17. Artificial Immunity in Experimental Tuberculosis. N. Y. Med. Jour., 1903, July 18; Trans. Assn. Am. Phys., 1903, XVIII, p. 97.
18. Two Experiments in Artificial Immunity against Tuberculosis. Med. News, 1905, September 30; Trans. Nat. Assn. for Study and Prevention of Tuberculosis, 1905.

DR. JANEWAY: That the president of the New York Academy of Medicine should have left his busy life to come down here to speak to us; that the dean of the Saranac school should have made that long journey to tell us of his knowledge of Dr. Trudeau's career, are eloquent testimony to the esteem and love which Dr. Trudeau, the man, inspired in all who were with him. It has undoubtedly been difficult for the previous speakers to confine themselves strictly to the subject assigned to them.

The last speaker needs no introduction to you, and though he comes but from across the city, he faces no easier task, for his relations with Dr. Trudeau were very close. He will not, however, have to confine himself to any phase of the subject, but will, we all hope, tell us in an intimate way of those sides of Dr. Trudeau's character which, whether we be physicians or not, must appeal to all that is best in us, and must stimulate us to lead in our turn lives which will awake in others those same feelings of respect and of affection which were so universally aroused in everyone who came into any contact with the great physician of Saranac.

DR. TRUDEAU, THE MAN.

BY HENRY M. THOMAS.

It is not easy for me to speak at this time of Dr. Trudeau as a man. He meant more to me than my emotions would stand should I endeavor to tell you. Perhaps the very closeness of our relations obscures my perspective and unfits me for the task.

The oft-quoted saying that "no man is a hero to his valet" did not apply to Dr. Trudeau; indeed, the closer one got to him, the more one admired and loved him. No one who served him ever voluntarily left his service. Dr. Trudeau's power of understanding and sympathy, and his many-sided nature, made it easy for him to get close to a very great variety of people, and if I tell you of my first meeting with him at Saranac Lake, I may be able to give you some idea of his personality, and how simply and unconsciously he allowed it to influence those who came near him.

Just 28 years ago, almost to the day, I went to Saranac Lake. That sounds a simple enough statement now, for many have since traveled the same route. But at that time it may be said that Saranac Lake was practically unknown to the medical profession in Baltimore; actually, I was the second person from here who had ever gone there in search of health. Dr. Trudeau, in his autobiography, gives a graphic picture of the mental state of one who has been ordered to stop work and go away, but I do not believe that it can be understood until it has been experienced.

The circumstances in my own case were such as for the time to abolish any particular desire to live. Dr. Welch, who was my medical adviser in Baltimore, encouraged me in many ways, and assured me that I should be able to continue my work in neuro-pathology in Dr. Trudeau's laboratory, for he was sure that he had a laboratory, as he had done such good laboratory work. Dr. James in New York cheered me on my way, but although I was not very ill, my nervous depression was such that I took three days to make a trip that might have been accomplished in 24 hours. A narrow-gauge railroad from Plattsburg had just been completed to Saranac Lake a few days before, and, if anything could have added to my depression, it was the desolate journey through the snow.

However, it was during this trip that I first became aware of Dr. Trudeau's influence as a man. As I remember it, the only other passenger in the car was a Mr. Krumholtz, who spoke to me and told me something of the place to which we were going. I gathered from him that Saranac Lake and Dr. Trudeau meant practically the same thing, and that the doctor would make it all right for me.

Mr. Krumholtz was my introduction to that ever-increasing company of those whose tuberculosis has been arrested by following the trail blazed by Dr. Trudeau, and I shall never forget his cheerful, helpful talk.

Dr. Trudeau had, in response to a letter, arranged for me to live at the Berkeley, which was then the largest boarding-house for his patients, as well as the village hotel. The talk at dinner on that day among the 15 or 20 guests was of the kind most interesting to lungers, and I thought how depressing such a conversation might be to a non-medical novice. Some of them had during the morning been examined by the doctor, whom they called "The King," and it was not hard to see from the way they spoke and the others listened, and from the questions that were asked, that I had, indeed, come into the domain of a ruler whose word was cheerfully accepted as the law of the land.

That afternoon, while I was unpacking, Dr. Trudeau came to see me. He was a tall, straight man, weighing more than 180 pounds, and his very dress typified the woods and all outdoors. At his heels was his companion, Nigger, a black mongrel, who went everywhere with him except hunting and to church. Dr. Trudeau would not take him hunting, and he never made any attempt to follow him to church.

I am unable to remember what he said to me on that first afternoon, but looking back at it now I think of that interview as the beginning of a friendship that has meant more to me than I can express. I remember this, that in a little while Dr. Trudeau took me out with him, showed me the village, and then left me with a group of young people—patients and their friends—who were tobogganing.

The next morning, which was Sunday, he sent his guide over with a note, saying he did not consider it right that a young doctor, engaged to be married, should eat his Sunday dinner alone, and asking me to take dinner with Mrs. Trudeau and himself.

From that time on I became Nigger's rival, and was with Dr. Trudeau as much as possible; indeed, I both hunted and went to church with him.

Dr. Trudeau, when I first knew him, was 40 years old, and his health was as good as it ever became. Indeed the first impression that he made on me, of a man in vigorous active health, abounding in energy and love of life, was only confirmed by a more intimate knowledge. He was always busy, and the only physical limitation that his disease seemed to have left was his inability to take long walks, skate, run, swim, or row.

His days were full, and he passed quickly from one thing to another, giving to each enthusiastic attention. At this time Saranac Lake was a compact little village in which we were all thrown very closely together. A single stranger in town, or, indeed, a new dog, created remark and had to be explained. There were not many very sick people among us, and as Dr. Trudeau examined his patients only very rarely, some of them only when they came in the fall and when they went out in the spring, and visited them only when there was special need, his winter practice at this time was not very exacting.

The sanitarium was three years old and then contained about 30 patients. The entire management was on his shoulders and, of course, occupied a great deal of his time. He had started almost unconsciously a wonderful institution, and it was amusing to see his assumed consternation at its growth. He would half-jestingly complain bitterly of the load he had to carry, while grasping with avidity every possible chance of increasing it.

He worked some part of every day in his laboratory, which consisted of his narrow office with a boot closet at the end, his barn, and the pit that he had had dug in his backyard. As you can well imagine, the hope that Dr. Welch had encouraged in me that I might continue my own work at Saranac Lake vanished the moment I went with Dr. Trudeau into his office, and very soon the desire as well, for after being with him for a

little while I had no other thought than that of doing what I could to help him.

Just at this time many cures for tuberculosis were being advanced, most of them based on the supposed germicidal action of various agents. He tested these and many other things in the hope that he could find something that would kill the organism within the body.

A French observer had stated that men who etched on glass with hydrofluoric acid seldom had tuberculosis, and it was supposed that it was breathing the fumes of this chemical that accounted for the supposed effect. This we tried, and it did, indeed, kill the germ in culture. I saw that he would like to try it on a patient as well as on inoculated animals, and I suggested that I try it on myself. He was somewhat loth to let me do so, but finally consented, and thereafter I sat for two hours a day in a room breathing the fumes of hydrofluoric acid, and with the result that every bit of the glass in the room was etched and that the bacilli disappeared from my expectoration. The rabbits did not fare so well, and although one or two other patients tried it, no further result was obtained.

The deftness and skill which Dr. Trudeau showed in all his actions were very evident in his laboratory technic. Even though the apparatus was simple in the extreme, it was nicely adapted for its purpose and was used by a master workman. One thing in particular I should like to mention, as it never failed to arouse my astonished admiration. The thermostat was heated by a kitchen coal-oil lamp, and Dr. Trudeau regulated the temperature by turning the flame up or down, and opening one or more of the doors of the wooden cases that surrounded the tin box. This was not hard to do during the day while the fires in the house were kept up, but it required skill to arrange for the whole night when the fires went out and everything was apt to freeze. Before going to bed he would look at the barometer on his table, go out-of-doors and look at the thermometer, make an observation of the heavens, and as a result he would turn the flame up or down, and shut or open the various doors. In this way he was able to keep the temperature of the thermostat within the proper limits. How successful he was is shown by the fact that at Saranac Lake living cultures of the tubercle bacillus could even then always be found, and this could be said of nowhere else in the country.

Dr. Trudeau constantly over-estimated other people's attainments and knowledge, and I discovered later that he had looked forward with high expectations to my arrival, as a man who had worked with Dr. Welch in the laboratory of The Johns Hopkins University. His disappointment must have been keen, for I knew little more about bacteriology than I did about hunting.

In later years, when I went with him to the meetings of medical societies, I was often amused, when he had approached some noted physician and told him of the experiments that he was doing and asked for advice about some troublesome point, to note his astonishment when he discovered that it was he himself who was regarded as the authority, and that the other physicians had nothing to offer except respectful attention.

This modest estimate of his own position in the medical world he showed also by requesting all of his patients to be examined frequently by Dr. Loomis or some other consultant to have his opinion confirmed, and he only gave up this habit when the patients flatly refused to regard any other advice than his as necessary, and when he discovered that more and more patients were taking the trip to Saranac Lake to get his confirmation of the advice that had been given by others.

Dr. Trudeau tells in his autobiography how much pleasure he took in getting money for his sanitarium and his other charities; indeed, he put into this quest much the same spirit and craft that he used in hunting game. He does not tell, however, how persistently he shrank from any personal profit that might accrue to himself from the growing reputation of the institutions at Saranac Lake. He, of course, could not avoid charging his private patients, but his fees were always most moderate and usually absurdly small, and at no time commensurate with his reputation. Many of his patients were among the very rich, and at the height of his activity the number who applied to him was very large. He distributed them lavishly among the increasing group of younger doctors who had collected about him, and if I told you the largest sum that he ever made from his practice in one year you would find it difficult to believe me.

Although I am sure that every one of us who was ever associated with Dr. Trudeau in his practice would have been glad to have followed the usual custom in such cases and worked on a percentage basis, that was not his way; he never divided fees, the other man got them in full.

He was unwilling to profit by the work of another man, and he carried this unwillingness to such an extreme that it was at times a check to our offering to assist him. I remember on one occasion I was with him when he prescribed for one of the guests at Paul Smith's. His medicine bag was over at his cottage, and he asked me to get it, and then tried to make me take the fee.

It would be impossible to tell you of his endless generosity to everyone about him. He did, indeed, learn how to beg, but he was a born, incurable giver.

He was extremely sensitive to the groundless fear that some one might think that his advice to patients to remain in the Adirondacks might be controlled in any measure by any possible pecuniary advantage to himself.

The rapid development of Saranac Lake village offered tempting opportunities for investments in real estate, but Dr. Trudeau would never have the least interest in any of the boarding-houses or the many houses that were built for rent to the patients, and, in fact, he got rid of a large tract of land, at no profit to himself, upon which the most extravagant section of the village now stands.

He refused many suggestions by astute business men to become interested in the establishment of private sanitaria. One of these suggestions that particularly aroused his wrath was an offer of $10,000 a year for his nominal direction of an institution and the use of his name on the circular.

The patients at the sanitarium have always paid much less than it costs to maintain them, and as the sanitarium grew, the annual deficit, as well as the expenses, increased in proportion. It was evident to Dr. Trudeau and to everyone else, that if he would accept well-to-do patients and charge them as they were charged at other sanitaria, the much needed money would be assured. The idea of such a change was never entertained, and Dr. Trudeau kept the institution exclusively for the benefit of those for whom it was designed.

Medicine was not a business to Dr. Trudeau, nor was its study a fascinating response to scientific curiosity. The central, compelling force was a strictly humanitarian desire to do everything that he could to cure tuberculosis, or, if not this, to alleviate the condition of the sufferer as much as possible.

Once having assumed the care of the sick, and having asked and received assistance in his charitable and scientific work, he felt the responsibility very deeply, and gave himself to these objects with absolute unrestraint. It was distressing to him to feel that possibly he might not be measuring up to what he characterized as the "demands of the great public," and he often taxed himself far beyond his strength, in spite of all that his associates and friends could do to shield him. This was particularly so during the summer months when he had charge of the practice among the guests at Paul Smith's Hotel and the campers on the St. Regis Lakes, as well as the responsibility of the sanitarium and his patients at Saranac Lake. In the early years he lived during the summer in his camp on Spitfire, and was rowed to the hotel and about the lakes to see his patients. When he had to go to the sanitarium or village it entailed a drive of 15 miles. As he became more busy, he gave up his camp and lived in a cottage near the hotel.

I wish I could give you some idea of his incessant activities, and the multitudinous demands that were made upon him during these summer months. It was more than any one man could do, and he had to have assistance. I was fortunate enough to be the first to help him, and did so for a number of summers. On the days that he was at Paul Smith's it was possible to relieve the strain somewhat, but on what he came to call the "horrible Tuesdays and Wednesdays," when he went over to Saranac Lake, little could be done. Mrs. Trudeau always went with him on these trips. They left as early as they could and drove to the sanitarium, and then after luncheon to his office at Saranac Lake, where he saw and examined a seemingly endless stream of people who had collected from all over that region of the Adirondacks and come in from the outside to see him. His office hours were repeated again the next morning, and he also had to visit some of his patients who were bed-ridden.

They usually got back to Paul Smith's just before dark on Wednesday, and it was always with apprehension that I met them. Dr. Trudeau was often, as he himself described it, a wreck—absolutely worn out nervously. Time and time again we urged that patients who really wanted his advice would come to Paul Smith's or anywhere else to see him, and that it was wrong for him to put himself to this useless strain. We were entirely unable to convince him, and he persisted until his health made the trips impossible.

His modesty kept him from believing that many people would take the extra journey to Paul Smith's for his advice, and, more than this, he felt that these weekly trips were a duty to the growing community at Saranac Lake and its neighborhood, and especially to those whose finances would preclude their coming to Paul Smith's.

At the end of each summer Dr. Trudeau was usually wretched, at times with fever and other evidences of the renewed activity of his old process, but more often, at the time I am speaking of, as the result of the strain on his highly strung nervous system. He was subject from his boyhood to extremely sharp attacks of ophthalmic migraine, and was liable to such attacks at any time, but he was almost certain to have them when under any very special strain, as on his Tuesdays and Wednesdays at Saranac, and they were very frequent by the end of the summer.

As is not uncommon with such a make-up, Dr. Trudeau had a very remarkable power of going through with what had to be done, in spite of his feelings, on pure nerve, and paying for it afterwards, and it was only those of us who were very close to him who really knew how he suffered.

His intense interest in hunting and other sports was a great help to him at these times, and we always felt that if we could get him down to Little Rapids, and into the woods with his old guide and friend, Fitz Hallock, that his astonishing recuperative power would again restore the balance.

Soon after I first went to Saranac Lake I started hunting with Dr. Trudeau, and two or three times a week his guide, Parker, would appear at the Berkeley, while I was at breakfast, with a note asking me to come over to the house a little earlier, as there was something he had for me to do.

I soon learned that this "something" meant that there was a hunt planned, and I expected to find, when I got across the street, his sleigh at the door, with Bunnie, his favorite hunting dog, hitched to the back, and the guns hidden under the lap robes. There was always a certain amount of secrecy connected with these expeditions, and we got out of the village with as little fuss as possible, the idea being not to reveal the location of the rabbit swamps to the other sportsmen. It was a rare hunt that was unsuccessful, for we had along the best shot and the best rabbit dog in the woods, but it was a very rare hunt, indeed, in which I killed the rabbit.

Dr. Trudeau's patience with my clumsiness and awkwardness in the woods was boundless, but at times he could not refrain later from describing to Mrs. Trudeau in his inimitable way how I, by moving my feet, or taking the wrong position, or by missing an easy shot first with one barrel and then with the other, had managed to cheat Bunnie of the satisfaction of finding his rabbit dead at the end of a long and intricate hunt.

Dr. Trudeau was a keen hunter and an almost perfect shot, but even at this time his scientific work, his sanitarium, and his practice, were occupying so much of his time that he had become the physician who hunted for recreation instead of the

sportsman who occasionally prescribed for a patient because he had to. He had entirely given up fox hunting as that required too much time, but three or four times a week he and I would go into the woods and hunt rabbits for an hour or two. A good rabbit dog was something that he insisted upon having. He tried out every kind of hound that seemed promising, and Fitz Hallock always had two or three dogs in training, many of which were the result of experimental breeding.

At this time of his life he was rarely able to get the time to hunt deer, and it was not until later, when he became part owner of Little Rapids, a hunting preserve, that he did so with any regularity.

Next to hunting and fishing his chief sporting interest was in relation to sailing. He and Mr. Anson Phelps Stokes inaugurated the sail-boat races on the upper St. Regis Lakes, and were keen rivals for a number of years, but here again his professional duties had interfered by the time I became very familiar with the summer life of the Adirondacks, and he had turned this phase of his activities over to the efficient care of his son Ned. He always loved a good horse and always owned one.

His enthusiasm for almost all kinds of sports and his remarkable skill in most of them made it easy for him to get into close sympathy with many of his patients who cared but little for his other activities, and they took unquestioned his advice about their health and the management of their lives when they learned how good his knowledge was about the things that they themselves knew.

Trudeau the man, and that means the physician, the scientist, the philanthropist, and the friend, cannot be understood without some knowledge of his intimate home life. Fortunately, he has himself on many occasions, and most beautifully in his autobiography, acknowledged his dependence on the quiet, strong, ennobling, unselfish influence of his wife. Without Mrs. Trudeau I do not believe that Dr. Trudeau, as we knew him, would have been possible—she so perfectly supplemented his high-strung, emotional nature. Always, but especially so in times of discouragement, sickness and sorrow, she gave him with perfect understanding just the help he needed.

I cannot brave the attempt to describe the home life of the Trudeau family, nor to estimate the privilege it was to many of us to share it in some degree. Dr. Trudeau's life at home differed in no essential particular from his life that was open to the whole world; indeed, all of his activities were centered there, and it was there only that one was able to get a clear idea of all the various channels through which his influence went out to the public.

Mrs. Trudeau sympathized in all of her husband's aims, but she was, I think, the directing force in his work of building churches and maintaining them. Dr. Trudeau was a deeply religious man, but cared little for creeds or doctrinal theology, although he was a consistent Episcopalian. His sustaining optimism was based on his faith, and it was this that he believed enabled him to accomplish some of the things that his vision set before him. He has beautifully expressed this as a farewell message to the medical profession in his address on "Optimism in Medicine." He radiated this message of faith and optimism constantly to his patients and those about him, although he preached it but rarely.

Dr. Trudeau had a remarkable facility for letter writing, and the letters that he wrote were innumerable. When he was well and in full activity, he would occupy every spare moment with his correspondence. It made little difference to him how many people were in the room or what they were doing. One of the clearest pictures I have of him at home is sitting at his desk surrounded by a roomful of talking and laughing people, who would interrupt him constantly while he wrote letter after letter, throwing each finished one on the floor until the rug at his feet began to resemble the snow-covered world outside. For many years he had no secretary and wrote all of his letters himself; indeed, to the end of his life he acknowledged every contribution to the sanitarium with a personal note. He wrote a long letter to his mother every Sunday of the many years that they were separated, and even more frequently to his sons, Ned and Francis, while they were away from home being educated, and more rarely to other members of his family. He also corresponded regularly with a host of friends and patients.

I think there must have been very few sick people indeed who had been under Dr. Trudeau's care who ever stopped regarding him as their chief adviser. Certain it is that he was getting letters constantly from all over the world from former patients asking his advice and counsel about any contemplated step that seemed to them important. In some instances, where he had advised patients to leave the Adirondacks because the disease was so far advanced as to exclude all hope of its arrest, or for some other reason, he kept in intimate touch with these patients and managed to send them something of his sustaining personality.

Since Dr. Trudeau's death I have had sent me a series of letters he wrote to a patient, and I would that I could read them all to you.

This patient, a young woman, had gone to Saranac partly through my advice, but, unfortunately, the process in her case was steadily progressive, and Dr. Trudeau had from the first little hope that it could be arrested. The hopelessness of the condition stimulated, as it always did, his desire to help and, after she had left the Adirondacks with no thought of returning, he wrote to her frequently, intimate gossipy letters, but each containing some suggestion, either as to the details of her management, or some thought meant to help her bear the inevitable decline.

Once he wrote two long, patient letters, explaining to her the uselessness of a troublesome and somewhat quackish treatment that she had been urged to take by members of her family.

At another time, towards the end of her illness, he uses his authority to protect her from the well-meant but bothersome attentions of a nursing friend. I shall read you this letter.

Dec. 17, 1895.

Dear Mary,

I was very much amused at your letter and the cause of your trial. Indeed, it is just what I expected, but I refrained from expressing any opinion of the young lady because I understood she was coming as a friend as well as a nurse. Her letter was the most

impossible, impracticable kind of twaddle I ever read, and I would have sent it to you to laugh at had it not contained some inquiries about your ailments and yourself which would have annoyed you and which, of course, I did not answer. If she shewed you my answer you will see I didn't say very much! As I did not know her I hardly felt she had a right to write me about you and your affairs as she did. You can say most emphatically from me that I think a temperature chart is a very bad thing for you to keep, and I don't even like you to use a thermometer and never except when you feel sick enough to want to go to bed and need to know for that reason. The salt sponge is all right, of course, if you choose to take it. I wish I were there to lay down the law to your friend, and I can see she must be rubbing you up the wrong way all the time and making your illness all the harder to bear. You see I am just conceited enough to think I understand you a little. Don't let anybody bother you; it is against the doctor's orders.

And with love believe me,

Most sincerely,

E. L. Trudeau.

Dr. Trudeau never came very close to a patient without getting into touch with the patient's inner spiritual life, and it was an unusual person who was not better for having known him.

Another letter, evidently an answer to one which uncovered some of the innermost feelings of this patient's sensitive and reticent nature, is very characteristic:

April 13, 1895.

Your sweet letter to me was a great pleasure, and you may be sure I appreciate the confidence you have given me so fully. I will never say any more about your cold nature again, but I never meant any of it and only wanted the visible proof of what I knew so well was there all the time if I could only be so fortunate as to call it out.

I am very glad to hear you had so comfortable a journey and had a look at the pomps and vanities, as I think you need all of that side of life that comes in your way, for I have, I assure you, appreciated and felt all you have had to go through here and wished I could help you, but we must each walk the thorny path which leads through suffering, if we would learn to look over and beyond, and to know the peace which all the trials of life only intensify for us.

This patient was only one of very many to whom he wrote such letters, and if it were possible to collect them, they would make a lasting record of his treatment of tuberculosis and his management of patients who were suffering from that disease.

Dr. Trudeau never complained of the tax upon his time and strength which his personal correspondence demanded, but when his mail was full of letters from hopeless invalids demanding immediate admittance to the sanitarium, and from strangers asking advice, scientific or otherwise, and a host of other such things, he was apt to rebel. But nothing aroused his indignation so quickly as when some luckless correspondent intimated that he would gladly compensate him with money for some special privilege or service.

During the last years of his life Dr. Trudeau had the help of a stenographer, but even with this alleviation his correspondence was so great that some of his closest friends refrained from writing to him as often as they wished in order to spare him, and begged him when he wrote to send them dictated letters. Dr. Trudeau never learned to express himself with the same freedom and ease when dictating as when he wrote by hand. Nearly all of his scientific articles were so written and the whole of his autobiography, and during times of stress and illness he found comfort and relief to his emotional nature in writing unreservedly to his most intimate friends.

The writing of his autobiography was a great surprise to himself. He had refused many offers from people who wanted to write his life and had resisted the urging of friends that he should himself write down his own experiences, and only succumbed at last to the kindly insistence of Dr. James, and consented to make the attempt. He began with very little hope of having either the ability or strength to complete it, but, when he had commenced, his wonderful enthusiasm came to his aid, and as the work progressed he became conscious of powers that he had no knowledge of possessing. When asked how he could write so vividly of scenes long past, he said that he recalled them to mind largely as visual images and wrote describing what he saw as rapidly as possible. He wrote on small sheets, often erasing and correcting them until they were almost illegible, and from these his secretary made the typewritten copy which rarely required the least correction.

I am conscious how very imperfectly I have been able to convey to you any adequate idea of Dr. Trudeau's personality. His was a nature that appealed instantly to everyone who met him, and it was easy for him to influence even casual acquaintances. He used this power with great skill in controlling his patients, and getting assistance for his charitable work. Men and women gave him without stint their unremunerated labor, and others delighted to help him with their money. I have time after time watched for and seen this instant response to his personality as he met and talked with strangers. This power which he retained to the last is shown very beautifully in the account which Mr. Clayton Hamilton, in his recent book, "On the Trail of Stevenson," gives of his single interview with Dr. Trudeau. The trail had led Mr. Hamilton to Saranac Lake in the Christmas season of 1911, where he saw Dr. Trudeau, who was then ill and sitting out on his little porch. They talked about Stevenson and of Dr. Trudeau's own work. Among other reminiscences, Dr. Trudeau recounted, as he delighted in doing, Stevenson's remark on the one occasion in which he had been decoyed into the laboratory. Stevenson, after looking for a little while at the cultures and specimens, said about as follows: "Trudeau, we both are bearing lanterns, but I must say yours smells to me most confoundedly of coal-oil."

In relation to this story Mr. Hamilton writes:

The doctor told me this with humor; but it did not seem to me so funny when I thought about it afterward. At present I remember an eager, active-minded man sitting anchored in a lounging chair and muffled among furs; talking with that tense voice of the achieving dreamer; at home in life, though exiled from its laughing and delightful commonplaces; cheerful and alert, though slowly dying; young, clear-eyed, and still enthusiastic, although already ancient in endurance; lying invalided while his City of the Sick grows yearly to greater prominence among the pines; fighting with an easy smile the death that has so long besieged him, to the end that others after him, afflicted similarly, may not die.

And the best of our tricky and trivial achievements in setting words together dwindle in my mind to indistinction beside the labors and the spirit of this man.

Dr. Janeway: One impression, which I hope will be a lasting one, I think we will all carry away with us to-night. It is that, in our medical heroes it is impossible to separate the professional life and the personal character. That is not true in all professions, and certainly not true of all heroes. It lays upon those of us who are their successors in the medical profession to see to it that we are true both to their scientific and humanitarian ideals as physicians, and to the legacy of rich character and sympathy with suffering which they have left to us as men and women.

I am going to ask Dr. Thayer if he will express our thanks to the gentlemen who have so kindly come here to read these splendid tributes.

Dr. Thayer: May I first add a word about one phase of Dr. Trudeau's activities which has always impressed me deeply?

Dr. James has spoken of his diagnostic and prognostic abilities. These he used for years with wisdom and skill in determining just who, among the many patients who presented themselves as applicants for admission to the sanitarium, were most likely to be benefited by the advantages which it offered. As he has said himself, this was not an easy task; it was one in the exercise of which he was often criticised; but it was precisely those patients in whom the process was at its earliest stages, to whom the sanitarium was especially likely to give that help which might turn the scale. From this standpoint Dr. Trudeau exercised remarkable ability and discrimination. But often he went farther and exercised an insight and a charity so exquisite that those of us who have experienced it can never forget. He knew not only how to choose those whose lives were most likely to be saved; he knew how to choose those whose lives were most worth saving.

Some of us have had the experience of sending to him patients whose symptoms were not so favorable as, under ordinary circumstances, to justify admission to the sanitarium, yet patients whose character and circumstances were such as to appeal to us with peculiar force. We have sent him such patients without, perhaps, even a suggestion that they be admitted to the sanitarium. How quickly under these circumstances did his clear eye and his generous heart detect the great opportunity!

He who has seen the group of men and women gathered in that sanitarium can hardly have failed to realize that this was no ordinary collection of individuals, but a body of the elect. How many to whom all doors of hope seemed closed are now living happy and useful lives, thanks to the skill and insight and charity of this great and good man.

Those who knew Dr. Trudeau in his latter years were more and more impressed with the beauty of his face. It was not the beauty of line or of color such as one sees in the young, but a beauty brought out by crease and furrow and hollow, that beauty which character brings to age. Bradford tells of a well-known statesman that when spoken to of a certain man, he exclaimed: "I do not like him. I do not like his face." "That is not his fault," said his friend, "he's not responsible for his face." "Yes," he replied, "every man over fifty is responsible for his face." I have thought of that remark when looking into Dr. Trudeau's face. It was a wonderful face with a singular beauty and depth of expression. In the later days when sorrow and illness had settled upon him, it seemed sometimes as if the body had almost gone, as if nothing remained but the spirit which glowed in the light that shone from his eyes. The body has gone now, but the spirit remains, burning in the hearts of thousands of men and women whom he has taught to live, animating that fine body of students who are carrying on his great work at Saranac and handed on by them to us to-night.

We can hardly express deeply enough our gratitude to Dr. James and to Dr. Baldwin and to Dr. Thomas for what they have said to us. This is an evening that we shall all remember.

POST-PARTUM CARE OF THE PERINEUM.

By E. D. Plass, A. B., M. D.,

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The present treatment of the perineum both after uncomplicated labor and after primary perineorrhaphy appears to have descended from the antiseptic period, when any addition to the chemical armamentarium was hailed as a panacea. It would seem that obstetricians have been so absorbed in determining the proper time for, and the technique of, repair, that the old antiseptic after-care has been handed down unchanged to the present generation. Only occasionally a skeptic like Krönig has dared to doubt the value of these time-honored customs, but in the course of time all of us will be led to consider critically the value of antiseptics, and determine to what extent they are useful.

When it is realized that, with the exception of iodine, the usual antiseptics, in concentrations in which they can safely be used on the skin, destroy the usual pathogenic organisms only after a long period of activity, does it appear reasonable to expect that the ordinary irrigations and douches will have more than a mere mechanical action? The false sense of security engendered by the term "antiseptic" is dangerous, and when untoward results follow, many individuals seemingly prefer to lay the blame upon the solutions rather than upon their own slip-shod methods. To the average nurse the term "antiseptic" spells safety, and she trusts implicitly in the germicidal action of a 1:1000 or a 1:2000 bichloride solution.

By eliminating the use of such solutions we do away with this false sense of security, and in my experience the results will be as good or better than those formerly obtained.

There is practically no recent literature upon the routine post-partum care of the perineum, or the after-care of primary perineal repairs. In order to ascertain the general sentiment of the obstetricians of this country in this regard, various text-books of obstetrics were examined with the following results:

E. P. Davis (Manual of Obstetrics, 1914) advises "a copious irrigation by a small pitcher with an antiseptic solution," during the puerperium, and "pouring 1% lysol or sterile salt solution from a pitcher over the parts after each micturition or defecation and whenever the vulvar dressing has become soiled and has been removed," in the after-care of cases with suture.

Hirst (Text-Book of Obstetrics, 7th edition, 1914) says: "Care must be exercised to remove blood and blood clots from the vulva before putrefaction sets in. This is best done by placing the woman on a bedpan, letting a stream of boiled water run over the parts, and, if necessary, using cotton to wipe them off." No reference to any special care after perineorrhaphy was found; but since Hirst advises making all repairs at the end of one week, one can scarcely speak of primary repair.

Henry F. Lewis, in the Practice of Obstetrics, edited by R. Peterson, 1907, advises "irrigation with sterile water or sterile antiseptic solution, finishing by drying with some sterile gauze." He mentions no special care following repair.

De Lee (The Principles and Practice of Obstetrics, 1913), under "Aseptic care during the puerperium," says: "The patient is placed on a sterile bedpan, the nurse sterilizes her hands or wears sterile gloves, and gently pours from a narrow-lipped pitcher a solution of 1:2000 bichloride over the vulva. The excess is dried off without rubbing, by touching with dry sterile cotton . . . . Simple sterile water may be used." After repairs no special care is recommended.

Without exception the text-books examined recommended cleansing at frequent intervals with antiseptic solutions, with sterile water or with saline solution, and frequently the procedure was made very elaborate and time-consuming. There seems to be a widespread belief that some extra care is needed, but the variations in treatment have to do only with the choice of the solutions and in the method of their use.

In order to demonstrate whether such routine antiseptic treatment of the perineum after delivery has any distinctly beneficial effect upon the course of the puerperium or upon the healing of primary perineal repairs, the following clinical experiment was conducted. For a period of nine months—from November, 1914, to August, 1915—all patients were divided into two groups after delivery, A and B (every alternate patient being placed in group A), and treated as follows: Those in group A were given the routine perineal care and those in group B were given no special attention. The routine care consisted in bathing the vulva and perineum with cotton pledgets soaked in 1:2000 bichloride of mercury solution every four hours, as well as after each defecation and urination during the nine days the patient remained in bed. The patients in group B were merely kept macroscopically clean with warm tap-water and soap and a wash cloth. No attention was paid to voiding or bowel movements, unless, as sometimes happened after the initial dose of cathartic, the need of cleansing the parts was apparent. The bloody lochia were removed whenever necessary. Unless the patient was very ill, she was expected to keep herself clean. It was found that the average number of cleansings necessary was as follows: Four a day for the first three days, between the third and sixth days not more than two a day, and after this only one, at the time the morning bath was taken.

Every effort was made to eliminate any factors which might invalidate our conclusions. Since the two series were run simultaneously, the factors of nursing care and surgical ability in repairing the lacerations on the part of the house staff were removed as nearly as possible. The head nurse arranged the lists impartially and none of the staff knew how any particular patient was being cared for. No exceptions were made to the alternate grouping of the patients. At the end of the experiment, when each group contained 200 cases, the records were carefully tabulated and the following facts discovered:

TABLE A.

	Group A. Routine Care.		Group B. Soap and Water Care.	
Primiparæ	120	60%	102	51%
Multiparæ	80	40%	98	49%
Full term deliveries	181	90.5%	174	87.0%
Premature labors (7–9 months)	16	8.0%	21	10.5%
Abortions (under 7 months)	3	1.5%	5	2.5%
Spontaneous deliveries	185	92.5%	185	92.5%
Operative deliveries	15	7.5%	15	7.5%
Vaginal examinations	93	46.5%	92	46.0%
Temperature never over 100.4° F.	121	60.5%	128	64.0%
Temperature over 100.4° F.	79	39.5%	72	36.0%
Elevation due to uterine infection	30	15.0%	29	14.5%

Reference to Table A shows that the two series are quite comparable as regards operative deliveries and the frequency of vaginal examinations, the two factors which might possibly have had a bearing on the results. The total morbidity (temperature over 100.4° F., taken every four hours) is approximately the same (group A, 39.5%; group B, 36.0%); nor is there any striking difference in the incidence of elevation of temperature attributable to uterine infection, as determined by uterine cultures or by the clinical course of the puerperium (group A, 15.0%; group B, 14.5%). The advantage, if any, is in favor of the series not especially treated (group B).

The low percentage of vaginal examinations is attributable to the fact that the staff attempts to follow the course of labor by means of abdominal palpation and rectal examination, so that ordinarily vaginal examinations are made only for the purpose of instructing students. These, however, probably account, in part at least, for the rather high morbidity, particularly that referred to uterine infection. Fortunately, none of the infections were severe, and there was no mortality.

The number and character of the repairs of lacerations, as well as the conditions which obtained at discharge (12 to 14 days after labor) are shown in Table B. No particular scheme of repair was insisted upon, but all perineorrhaphies were made with catgut and silk-worm gut, either singly or in combination. Very small nicks were frequently not repaired.

TABLE B.

	Routine Care.				Soap and Water Care.			
	Well healed.		Poorly healed.		Well healed.		Poorly healed.	
1st degree...	39	92.8%	3	7.2%	29*	96.6%	1	3.4%
2d degree...	21	75.0%	7	25.0%	19†	95.0%	1	5.0%
3d degree...	0	.....	0	.....	1	100.0%	0	.....
Totals....	60	85.7%	10	14.3%	49	96.1%	2	3.9%

*One patient died eight hours after delivery (eclampsia).
†In one case, no repair (eclampsia).

It will be noticed at once that there is a considerable difference in the results obtained. The routine care was followed by many poor results, whereas excellent results were obtained where no antiseptic precautions were taken. Of special interest is the one case of complete laceration—a forceps delivery of a child weighing 5000 g. in the case of an 18-year-old primipara. She chronologically went into the "no care" list, and the supreme test was made. The healing was *per primam*, and not only was the sphincter tone excellent, but the perineal body was also in good condition. When one sees a poor result in only 12 out of 400 cases there is a tendency to be satisfied; but a comparison of the results obtained in the two groups—14.3% and 4%, respectively—clearly indicates that too many failures occurred in group A.

This method of treatment was so successful that on August 1, 1915, the old routine was entirely abandoned. Since that time 57 primary perineorrhaphies have been done as follows: 1st degree, 32; 2d degree, 17; and episiotomies, 8, with only three bad results (5.25%), and these were confined to the first degree tears. The episiotomies were all lateral, single or double, and the wound healed *per primam*.

To date we have treated 108 repairs in this manner and have had five failures, a percentage of 4.6.

In addition to the better results obtained after the elimination of the more or less complicated antiseptic treatment, there is an economy in the time of the nurses. All who have to do with nursing in large institutions will welcome any change which will free the nurses from the burden of unnecessary routine and give them time to really nurse the patients, provided the result is satisfactory.

Conclusions.—The use of antiseptic solutions in the care of the perineum during the puerperium or in the after-care of primary perineorrhaphies is of no value.

Macroscopic cleanliness alone gives better results and effects a considerable saving of time.

NOTE.—Since November, 1914, the antiseptic after-care of secondary perineorrhaphies done on the gynæcological service has been discontinued and no attention is now paid to the wound in these cases. Dr. V. N. Leonard, resident gynæcologist, has assured me that the results are excellent; and that, although no statistics have been compiled, the percentage of poor results is even less than previously. The absence of bleeding makes it unnecessary to resort to any special cleaning of the perineum so that the only attention consists of the usual morning bath. Vulvar pads are discontinued after two days.

THE CASES OF GAUCHER'S DISEASE REPORTED BY DRS. KNOX, WAHL AND SCHMEISSER.

By F. S. MANDLEBAUM, M. D., and HAL DOWNEY, PH. D.

(From the Pathological Department, Mount Sinai Hospital, New York, and the Histological Laboratory, Department of Animal Biology, University of Minnesota, Minneapolis.)

In the paper by Brill and Mandlebaum on Gaucher's Disease (1913) an attempt was made to show that this disease is a distinctive clinical and pathological entity, easily differentiated from a large group of diseases whose chief characteristic is an enlargement of the spleen. These authors called attention at the time to a considerable number of cases quoted in the literature as instances of Gaucher's disease without sufficient clinical or pathological data to warrant the diagnosis, and hoped that the subject would not be confused in the future by the addition of other unproven cases.

In the paper by Drs. Knox, Wahl and Schmeisser, which has just appeared in the BULLETIN of The Johns Hopkins Hospital,[1] two cases of Gaucher's disease in infants are reported, which, in our opinion, should not have been so classified. We are able to show that the diagnosis is unwarranted, not only by our analysis of the histo-pathology and microchemical tests as reported, but also because we had an opportunity of studying a portion of the material which was kindly sent to us by Dr. Wahl in June, 1914. At that time we came to the conclusion that the case (Case I) was not one of Gaucher's disease and called the attention of Dr. Wahl to this fact.

In an analysis of recently reported cases of Gaucher's disease, in a paper now in press in the *Folia Hæmatologica*, we decided not to comment upon the cases which were reported by Knox and Wahl in a preliminary communication while our article was in preparation, but preferred to wait until their full report appeared.

From the very limited number of cases of Gaucher's disease reported in infancy one is not warranted in describing a constant or definite clinical picture, for the disease at this age has

[1] Vol. XXVII, Jan., 1916.

usually been complicated by intercurrent affections of one sort or another. Still, one must bear in mind the fact that, whenever a reliable history is obtained, it will be found that most, if not all, cases observed in later periods of life, have originated in infancy or childhood. Inasmuch as one of the chief characteristics of Gaucher's disease is its essential chronicity, it must not necessarily be considered a fatal disease when occurring in infancy, even though the individual may succumb to some complicating affection. For these reasons it is unnecessary to enter into a discussion here of the clinical features at this period of life, in which the diagnosis may be difficult if not impossible.

We admit that the histo-pathology in any case of Gaucher's disease at its very inception would probably show but few cellular changes. Inasmuch as no autopsy findings at this earliest stage have ever been reported, we may assume that the diagnosis might be impossible. In the case just described by Drs. Knox, Wahl and Schmeisser, however, the cellular process is so far advanced that the normal appearance of many of the affected organs is almost entirely effaced. One might reasonably expect, therefore, to find at least some resemblance to the lesions of Gaucher's disease in a process so well established and so widespread in its manifestations. This, however, is not the case, as will be shown later. Moreover, in the case reported by Niemann, in which the disease began at the age of two months, the lesions 15 months later were limited to the hematopoietic organs and the histo-pathology is that of Gaucher's disease. Furthermore, the patient of Erdmann and Moorhead, upon whom a splenectomy was performed at the age of three years and four months, presented an enlarged spleen at the age of 14 months, and here also the histological examination shows the characteristic changes of Gaucher's disease.

From a study of the complete autopsy material in three of our own cases ranging from the age of four and one-half years to 42 years, together with the findings of six other observers whose cases also came to autopsy, we find it difficult to believe that a definite pathological process, limited invariably to the hematopoietic organs, could have originated in infancy or childhood as a widespread lesion involving the heart, lungs, pancreas, kidneys, adrenals, intestines, thymus, brain, and other organs. It is equally difficult to realize that the lesions described by Drs. Knox, Wahl and Schmeisser, which have no resemblance whatever to those of Gaucher's disease, could become localized in later life to the hematopoietic organs and conform to the well recognized picture of this disease.

In considering the histo-pathology of these cases we shall confine ourselves entirely to the results of our study of material from Case I. This consists of spleen, liver, lymph node and adrenal body. In the first place, it is quite evident that all sorts of tissues are involved in the process, particularly the parenchyma of the various viscera, as is freely admitted by the authors. This constitutes one of the most marked differences between these cases and the authentic cases of Gaucher's disease. In the liver there is no question but what the majority of the large, vacuolated cells are derived from the hepatic cells. A few vacuolated cells are seen in the periportal connective tissue, but there is practically no increase in the amount of interstitial tissue, and consequently the number of large cells from this source is very insignificant. The liver from a case of Gaucher's disease presents a totally different appearance. It shows a tremendous increase in the amount of interstitial connective tissue, both perilobular and intralobular. This interstitial tissue is crowded with the characteristic Gaucher cells, and it is quite evident that these have not been derived from the hepatic cells. Although the hepatic trabeculæ are more or less broken up by the invasion of Gaucher cells, the liver cells remain quite normal in appearance. In the case under discussion, the epithelial cells of the larger bile ducts are very much vacuolated, and conspicuous vacuoles are also seen in some of the endothelial cells of the larger vessels and also in a few of the stellate cells lining the sinusoids. In Gaucher's disease, even in the most advanced stages of the process, such changes in the bile ducts, sinusoids and endothelial cells of the vessels were never seen. The process is limited strictly to the interstitial tissue of the liver.

In the lymph nodes there are many differences between Gaucher's disease and the case of Drs. Knox, Wahl and Schmeisser. In the latter, the node is packed rather uniformly with the large, clear, vacuolated cells, although in parts of the node there is some grouping of these cells into rounded masses which are surrounded by sinuses or thin layers of connective tissue. These rounded masses seem to be medullary cords which have become rounded owing to the accumulation of foam-cells. In other parts of the node the large cells are quite uniformly distributed. In Gaucher's disease this grouping of the characteristic cells in rounded masses is much more conspicuous than in this case.

The long strands of modified tissue described by Bovaird, Risel, and by the writers in their forth-coming paper, are not present in this case. These long strands, which show clearly that the characteristic cells are derived from the reticulum, are very characteristic for Gaucher's disease, and their total absence in the present case is a strong argument against classifying the case with Gaucher's disease. Another feature which does not correspond to conditions in Gaucher's disease is the reduction in the amount of the reticular tissue of the node. To a certain extent this is replaced by many strands of young white fibrous tissue which grow into the node from the capsule, carrying many thin-walled vessels and capillaries with them. The reduction in the amount of the reticulum makes these capillaries appear very prominent. Such a condition has not been reported in Gaucher's disease.

In the node available for our study the process was so far advanced that it was impossible to determine the source of the large cells. However, the presence of a few abnormally vacuolated lymphocytes seems to indicate that some of the large cells are transformed lymphocytes.

In the spleen the pathological process is just as far advanced as it is in the early case of Gaucher's disease which has already been reported by one of us. Although the organ is packed with large, clear cells, there are many points of detail wherein it

differs from the spleens of the Gaucher cases reported. The enormously enlarged sinuses surrounded by rather thick connective tissue walls, which are so characteristic of the Gaucher spleens, are not seen here. The photomicrographs, figures 1-4 published in the paper under consideration, when compared with those published by Mandlebaum (*Jour. Exper. Med.*, Vol. 16, plates 78 and 79), show the difference clearly enough. In the Gaucher spleen there are a few normal sinuses, but most of them are greatly modified and they are usually without an endothelial lining, although they may be lined with one or two layers of Gaucher cells. In the case under discussion, although the process is far advanced, most of the sinuses are fairly normal in size and appearance, except for the fact that their endothelial cells are usually somewhat swollen.

In the Gaucher spleens the characteristic cells located between the sinuses are arranged in the form of rounded solid masses, or as alveoli, which are surrounded by thick bands of fibrous tissue. Some such arrangement occurs in the spleen of the case under consideration, but the masses are not nearly so clearly outlined as is the case in Gaucher's disease. In other words, the organ is much more uniform in appearance than is the case in Gaucher's disease. The large cells are similar to those of the lymph node and liver, but they are very different from the characteristic cells of Gaucher's disease.

In the adrenal body it is clear that all parts of the organ are affected by the process, the cells of the zona reticulosa most. Cells of the zona glomerulosa are vacuolated, and also those of the zona radiata. In the lower portion of the latter zone the nuclei are small and irregular, but the cells are not materially enlarged in these two regions. The cells of the zona reticulosa show the greatest amount of change. These cells are very large and very much vacuolated, and they have the same characters as the cells in the other organs. Rounded groups of these cells are surrounded by bands of fibrous tissue, and in this respect the adrenal bodies suggest Gaucher's disease more than do any of the other organs examined.

The cells of the medulla are also affected, but they have not enlarged to the same extent as those in the zona reticulosa. They are still quite dark and can be recognized as medullary cells. Their nuclei are fairly normal.

It is perfectly evident from the above, as was also pointed out by Drs. Knox, Wahl and Schmeisser, that the large vacuolated cells are derived from the parenchyma of the organ, chiefly the zona reticulosa. But that part of the organ which one would expect to find modified, if this were merely an extension to the adrenal body of the characteristic pathological process of Gaucher's disease, namely the supporting framework, shows little or no change, excepting that it is slightly increased in amount. In Gaucher's disease, no matter how far advanced the process, the reticular tissue of the hematopoietic organs and the supporting tissue of the liver are the only tissues involved.

It is claimed that the large vacuolated cells, which characterize the organs of this case, are identical in structure with those which have been described for Gaucher's disease. Even a superficial examination shows that this is not true. The nuclei are quite similar in structure and shape, although multinucleated cells are far more frequent in Gaucher's disease. In other respects, however, the cells are quite dissimilar. In Gaucher's disease the large cells present a very peculiar appearance which is not exactly duplicated in any other pathological process. The stainable portion of the cytoplasm occurs in the form of a network composed of rather fine fibrils, the main strands of which take a more or less parallel, wavy course, in the direction of the longitudinal axis of the cell. Even in the more rounded cells the fibrils show a tendency towards a more or less parallel, wavy course across the cell, in case its nucleus is eccentric; or they encircle the nucleus, if the latter is near the center of the cell. In other cells the course of the fibrils is more complicated, but the main strands of the network show the same tendency towards parallelism. This gives the cells a peculiar streaked appearance which is very characteristic for Gaucher's disease, and which distinguishes its characteristic cells from those of the case under consideration and from those figured by Schultze and Anitschkow. Frequently the bands of fibers appear crowded apart in such a way that they form a rather distinct wall around elongated or irregular colorless spaces. Rounded, vacuole-like spaces are very infrequent, which is another point of distinction between the cells of Gaucher's disease and those of the lipoidemia cases and the case of Drs. Knox, Wahl and Schmeisser. These differences are brought out very clearly by a comparison of the figures published by Anitschkow (*Ziegler's Beiträge*, Bd. 57, Plate 8) with Risel's Figure 2 on Plate 6 (*Ziegler's Beiträge*, Bd. 46).

The large cells of the case of Drs. Knox, Wahl and Schmeisser are seen to be identical with those figured and described by Anitschkow, Schultze and Lutz, but very different from the cells seen in true Gaucher's disease. The cells are more rounded than are most of the Gaucher cells, and their cytoplasm is filled with rounded vacuoles—a condition which is very rare in Gaucher cells—and the longitudinal striations are absent. Since the presence of lipoids was demonstrated in these cells and in those from the lipoidemia cases of Schultze and Lutz and in the experimental animals of Anitschkow, it seems evident that this type of cell is characteristic for lipoidemia, but not for Gaucher's disease. These cells are never fused into greatly elongated strands and syncytial masses as is the case in Gaucher's disease (Risel, Bovaird). This constitutes one of the most important differences between the two types of cases and one which, with the streaked appearance of the cytoplasm of the Gaucher cells and the preservation of the non-stainable portion of the cytoplasm after treatment of the tissues by the usual histological methods, permits unfailing diagnosis of Gaucher's disease, especially when it is remembered that the supporting framework of the hematopoietic organs and of the liver are the only tissues involved in the process.

Let us consider for a moment the microchemical examinations which are reported in the first case only. It is stated that the substance in the large cells disappears on treatment with absolute alcohol or ether and is slightly refractile. The staining reactions with Sudan III, Nile blue, neutral red,

Ciaccio's method, and the Weigert-Pal myelin sheath stain, while not altogether convincing as to the specific type of lipoid matter present, give certain reactions. Anisotropic bodies are seen after the myelin sheath stain. It is stated that these reactions suggest the presence of some fat-like substance that may be in the process of transformation into neutral fat and may represent an intermediate stage between the latter and a closely bound molecule of proteid and fat. In other words, some substance of a lipoid nature, resembling myelin and closely related to the fats, is found by microchemical tests.

The material which Dr. Wahl kindly put at our disposal had previously been placed in alcohol, therefore we were unable to make microchemical tests for lipoids. The opportunity of studying the lipoid question, however, was afforded us through the kindness of Dr. Bernstein, from whom we obtained material from two cases of Gaucher's disease in children. Our examinations were made on frozen sections of fresh spleen as well as on material fixed in formalin. The complete results of our studies will appear shortly in the *Folia Hæmatologica*, but at present we wish to emphasize the fact that alcohol and ether have no solvent effects upon the substance contained in the large cells. On the contrary, they produce a rather characteristic reaction and make the substance more prominent. No appreciable differences in staining reactions between frozen sections of fresh or formalin-fixed material and the material embedded in celloidin are seen, indicating that the substance remains in the cells after embedding. For these reasons we have considered that this substance may be of protein nature. Sudan III, Scharlach R, Nile blue, neutral red, and osmic acid were all negative, as well as the methods of Smith-Dietrich, Fischler, Ciaccio, and Golodetz. Weigert's myelin sheath stain was also negative, and overstaining with Weigert-Van Gieson, as suggested by Schultze, failed to show a reaction. Furthermore, no anisotropic bodies were demonstrable with the polariscope. Consequently we felt justified in stating that when neutral fats, myelin substances or anisotropic bodies are found by *microchemical methods* in the cells of any process resembling Gaucher's disease, the latter disease may be excluded.

We wish to say here that we fully realized that an appreciable amount of fats and lipoids might be present even though our microchemical tests were negative; therefore, a complete chemical analysis was made. Our results led us to believe that the substance in the cells of Gaucher's disease is not of a pure lipoid nature, but of protein nature possibly in combination with the phosphatids. Any definite theory, however, in reference to the exact nature of this substance is as yet speculative.

In order to avoid any misconception regarding Niemann's case—for Drs. Knox, Wahl and Schmeisser would have us believe that their cases are of the same type—we wish to emphasize that the lesions in Niemann's case were confined to the spleen, liver and lymph nodes (the bone-marrow was not examined by them), and the histological examination showed the typical picture of Gaucher's disease. Furthermore, no reaction with Sudan III was found, the sections presenting simply a dirty, reddish tinge.

Drs. Knox, Wahl and Schmeisser also believe that the spleen described by Schultze in his case of diabetes accompanied by lipoidemia is like those found in their cases, and consider it very probable that Schultze's case "represents an earlier stage in the same process." It may be stated that Schultze's case bears no resemblance whatever to Gaucher's disease. It was a case of diabetes mellitus associated with large cell hyperplasia of the spleen, the cells showing certain reactions for lipoids. Such a comparison, therefore, is of no importance in corroboration of the diagnosis of Gaucher's disease.

Since Schultze's presentation at the meeting of the German Pathological Society, two cases of diabetes of similar nature were reported by Lutz, and through the kindness of Drs. Williams and Dresbach we have recently had an opportunity of studying the material of another case of diabetes, in which large swollen reticular cells filled with fat, lipoids and cholesterin esters were found. The histo-pathology, however, was not that of Gaucher's disease.

Drs. Knox, Wahl and Schmeisser make the following statement: "Schultze showed that the sections of the spleen of his case reacted to the microchemical tests for fats and lipoids just as did the sections of the generally accepted cases of Gaucher's disease described by Risel and Schlagenhaufer." This statement is not in accordance with the facts, for fats and lipoids were not present in the latter cases. In Risel's paper, on page 254, we find the following: "Dass diese Substanz nicht fettartig ist, geht aus dem Fehlen einer Schwarzfärbung durch Osmiumsäure hervor, ebensowenig färbt sie sich auch rot bei Behandlung mit Sudan III"; and also, "Bei der Untersuchung im polarisierten Lichte lassen die Zellkörper keine Doppelbrechung erkennen (weder im frischen Zustande noch nach Fixierung in Formol)." On page 129 of Schlagenhaufer's article we read: "Spezifische Färbungen auf Amyloid und Fett sind negativ. Doppelbrechende Substanzen sind nicht nachweisbar." Schultze studied the slides from the cases of Risel and Schlagenhaufer and noted certain resemblances to his case in the staining reactions, but he makes no mention of having examined their material for fats and lipoids. Schultze assumes, nevertheless, that there is a possibility of some lipoid substance in the cells. This assumption on his part is, of course, entirely unwarranted, as we have shown.

Let us turn for a moment to the question of cellular hyperplasia in general. The literature of the past few years contains quite a number of references to this subject, clinical as well as experimental. It is well known, for instance, that certain specific cells may react to the presence of toxins in the blood and give rise to hyperplastic and proliferative changes, such as are found in typhoid fever or tuberculosis. The injection of foreign substances into the circulation in vital staining also produces a reaction of the reticulo-endothelial cells, as was shown by Aschoff and Kiyono. The feeding of animals with fatty substances or cholesterin, as was shown by Anitschkow and others, is also followed by definite cellular changes in the same specific group of cells. In fact, the histological picture produced in these experiments is so significant that Aschoff has called the process "pseudo-Gaucher." We have seen that the

lipoidemia of diabetes may cause cellular changes in the spleen somewhat similar to those described in Gaucher's disease. Notwithstanding a certain similarity in all of these conditions from the biological and histological standpoints, the nature of the process, as well as of the substance contained in the cells, differs in each instance and depends on the underlying etiological factors.

Are we then to call every morbid process accompanied by the presence of "large cells" Gaucher's disease? Drs. Knox, Wahl and Schmeisser would have us do so, provided the cells contain lipoids, for they say: "Apparently any disease in which the spleen, together with any other organ, shows numerous large, pale granular or finely vacuolated cells, giving the characteristic microchemical reactions for lipoids and showing a tendency to be widely distributed, belong to this group, and any attempt to limit the condition to any single organ or any single set of organs is largely arbitrary."

It would be presumptive on our part to suggest the proper classification of the two unusual cases reported by Drs. Knox, Wahl and Schmeisser. That a widespread cellular hyperplasia, associated with degenerative changes in many organs, exists, cannot be denied. We are unable to suggest the etiological factors, toxic or metabolic, which have produced this unique condition, but we do insist that the cases are not to be classified with a well recognized disease, definite in its clinical manifestations and always accompanied by characteristic changes in a specific group of cells.

We have shown the many points of variance in the cases reported by Drs. Knox, Wahl and Schmeisser, when compared with Gaucher's disease. These authors, in attempting to classify their cases as instances of Gaucher's disease, have built up a theory, assuming some "lipoid metamorphosis," a theory which is not substantiated by scientific facts.

ADDENDUM.

Since the above was written, a paper by Drs. Wahl and Richardson has appeared in The Archives of Internal Medicine, Vol. XVII, Feb, 1916, entitled: "A Study of the Lipin Content of a Case of Gaucher's Disease in an Infant." The spleen and liver of Case I reported by Drs. Knox, Wahl and Schmeisser were employed for this work.

We wish to emphasize that Drs. Wahl and Richardson have misquoted certain fundamental facts, and their diagnosis and conception of Gaucher's disease, being built upon these misquoted premises, are necessarily erroneous.

In the first place, there is no authentic case of Gaucher's disease in the literature in which microchemical tests for lipoids have been definitely positive. On page 239 they say: "Marchand was the first to note the presence of a peculiar homogeneous substance within the cells which is dissolved in alcohol." We wish to say that nothing can be found in Marchand's article concerning the solubility of this substance.

Secondly, we are told on page 240 that both Schultze and Lutz found lipoids in the material from the cases of Gaucher's disease reported by Risel, Schlagenhaufer, and De Jong and Van Heukelom. This statement is not in accordance with the facts, for Schultze did not examine this material for lipoids, but simply called attention to the morphological resemblance of the cells in his case of diabetes to the cells of Gaucher's disease in the cases of Risel and Schlagenhaufer. Furthermore, Lutz, as well, called attention to the histological resemblance of the two conditions, but was unable to demonstrate the presence of lipoids in material from Schlagenhaufer's case. And, finally, neither Schultze nor Lutz made any microchemical tests on the material from the case of Gaucher's disease reported by De Jong and Van Heukelom. The latter authors, moreover, state that no fat, glycogen, amyloid or fibrin could be demonstrated in their case.

The present status of our knowledge of the chemistry of Gaucher's disease is too meager to warrant any lengthy discussion. An analysis of the figures presented by Drs. Wahl and Richardson shows that three different determinations were made on the same material with variations of more than four hundred per cent in their results. It is obvious that wrong conclusions will be arrived at if one averages, for instance, the figures of 2.5 per cent and 11.3 per cent on the same material.

We fail to see the justification of calling the acetone-insoluble portion of the extract "lecithin," without control nitrogen and phosphorus determinations.

We should like to call attention to the fact that Drs. Wahl and Richardson found a high percentage of total extractives in their case. A high percentage of extractives was also found by us in two cases of Gaucher's disease, but it is quite obvious that such findings may occur in the spleen in many pathological conditions. We do not think, therefore, that one is warranted in drawing any further conclusions from the chemical analyses reported by Drs. Wahl and Richardson.

THE RÔLE OF CERTAIN FLORENTINES IN THE HISTORY OF ANATOMY, ARTISTIC AND PRACTICAL.*

By Edward C. Streeter, M. D., Boston, Mass.

If among the cities of Italy Florence holds merely secondary rank as a medical center, or terrain, for anatomical studies, during the Renaissance, at any rate, the supreme achievements of her artists in the related realm of artistic anatomy more than made amends. Here she exerted a prepotent influence. The swift revolution in art resulting from the scientific studies of perspective and anatomy was largely the work of the Florentine "aurifabers" and artists. The typical Florentine man of temperament was a half-baked scientist; consequently, if he was also an artist, he spent much of his energy in perfecting his art on its formal side. He was a student with high and unquenchable appetencies, ready to use all the geometry that he could muster in solving problems of space, and all the anatomy that he could master in solving the problem of form. He mixed brains with his paints. Color was not his concern, but verisimilitude. He would sacrifice, at any juncture, grace and sentiment for verve and

* Read before The Johns Hopkins Hospital Historical Club, December 13, 1915.

vitality. His aim was utter fidelity to fact—in a word, realism. Now, a realist who recognizes in the use of the human form the supreme decorative principle may not necessarily be an adept in anatomy, but he will be an apt pupil. He will confess at once, that only by approaching mass beneath contour can we come to apprehend the human figure in its great outlines, the scale of parts, the mutually sustained mechanism. Anatomize you must—there is no alternative. To convey reality and a heightened sense of firm substance, dissection is the only way open to the serious man working, in the figurative arts, upon problems of the human form in movement. Roundness and undulation of body surfaces, certain characters of form and texture, it is true, may be conveyed without a deep-going knowledge of anatomy, but bodily action introduces a whole train of vital plastic complexities into the problem. Now, by the fourteenth century the Florentine figure artist was ready to cope with these difficulties. He simply had to cope with them. Before Giotto, the medieval Italo-Byzantine conventions required no knowledge of anatomy —all was flat, archaic decoration, submissive to outworn formulæ; there was no need here of applying myologic detail or mechanistic interpretation. But it is quite another matter when the great figurative arts bend to the task of portraying high emotional experience. To invest tense thought with sensible shape, to stimulate the moral will of the beholder, in fact, to tell a moving story convincingly and well in a representative form, at once religious and dramatic, required of the plastic artist that he unfailingly possess all that his forerunners had acquired in the knowledge of processes and materials, and that he keep abreast of his time in subjects such as the chemistry of colors, the mathematics of composition, the geometry of perspective, the illusions of chiaroscuro, and, above all, the science of human anatomy.

Provided he is well drilled in these disciplines, the artist has it in his power, in his very hand, to create a world of sensuous delightfulness framed for the spiritual uses of his kind. He *may* invest religious emotion with esthetic charm. He *must*, absolutely *must*, satisfy the imperious demands of secular art in the matter of faultless drawing and modeling of the undraped human form. From Masolino on, negligent drawing of the figure was a crime no maestro would be guilty of.

Dwell just for a moment upon certain incentives to the adoption of forms of art, frankly naturalistic, first observable in Florence in the fourteenth century. What social, civic or economic conditions gave to art on the Arno the specific direction of which we speak? Well, first of all, the new art was bourgeois by birth, and its appeal to the bourgeoisie must be fresh, simple, and immediate. It spoke unvarnished truth. It must avoid the region of abstractions and mystical conceptions, and cleave to the ponderable and measurable objects of sight and sense. In the second place, your realist directs his appeal to the honest, honorable intelligence of his fellows. He is close to their farthest-darting apprehension of the world of sense. Thus he becomes the protagonist of science, a self-appointed discoverer of truth. He is the first to feel the quickening to life of new faculties, of fresh intellectual interests, in his own milieu and time. He is a snapper-up of innovations, always on the alert for some novel way of illuminating the old substance of religious feeling. Symonds has something on this head :

As technical skill increased, and as beauty, the proper end of art, became more rightly understood, the painters found that their craft was worthy of being made an end in itself, and that the actualities of life observed around them had claims upon their genius no less weighty than dogmatic mysteries. The subjects they had striven at first to realize with all simplicity now became little better than vehicles for the display of sensuous beauty, science, and mundane pageantry. The human body received separate and independent study, as a thing in itself incomparably beautiful, commanding more powerful emotions by its magic than aught else that sways the soul.

Now, Art and Science during the Renaissance, "like two stars that held their motion in one sphere," kept trailing each other curiously. Particularly was this true in Florence, where the Medici encouraged all the scientific tendencies of Tuscan art. We have seen that the true creative power of the Florentine artist lay almost wholly on the formal side of his art. With the deepening of his scientific interests he applied himself with more fervent industry to the study of the technicalities of his craft. He appropriated inherited forms of expression more rapidly, and created new ones more readily. Always drawing was his chief concern—good realistic narrators and very few painters, in the strict sense of the word, came of Florentine blood. It is characteristic of him to make form alone serve as the living vehicle of the artistic idea. Indeed, he fashions his very ideas plastically. He uses his drawing pen, his modeler's stick, his chisel, casting-mould and chasing-tool, like so many instruments of precision: it is a foregone conclusion that he will use the dissecting knife, too, in the same service. The which he does.

I fear our notion of old Florence is somewhat jejune in one respect. We picture the city as an aggregation of deep gardens, and courtly precincts, all peopled with exquisites, whose discourse is neo-platonic nebulosity. The princes, the poets, the humanists are held too high in our evaluation. As a matter of fact, this city is the very paradise of little bank clerks who compose, not amorous rondels, but solid works on mercantile arithmetic; sound craftsmen who measure, balance, calculate, explore, just as Dante did, just as Giotto, Dante's friend, did, too, in his rude way. In Giotto are found the seeds of all future development of art in Florence—intimations of a vast abortive inquiry into the physical makeup of man, among other things. Among his followers is found still more curious research in this unblest quarter. His intimate (some say his kinsman), his intimate assistant Stefano is called "the ape of Nature." Billi tells us that this Stefano attained such a pitch of realism in drawing the human figure that the barber-surgeons, preparing to do a phlebotomy, would stand before his subjects and study the detail of the branching of the veins of the arms. His period is 1301-1350.

Reorganization of the study of nature was the issue of Giotto's teaching. The artistic training of a fourteenth century Italian "Primitive" was not so haphazard as we are

led to believe. Glance for a moment at the "Treatise on Painting," by a pupil of a pupil of Giotto—Cennino Cennini is his name, and he lived at the end of the fourteenth century. In Chapter 70, part 3, he says, speaking of the proportions of the human figure: "I will make you acquainted with the proportions of a man; I omit those of a woman because there is not one of them perfectly proportioned." A rude beginning and a rude treatise, but it is the best since Vitruvius. "Flesh tints," continues Cennini, "are laid on with verde terra and biacca; go twice over the naked parts. When painting the faces of young persons with fresh complexions this tint should be tempered with the yolk of a town-laid egg. . . . . You must prepare three gradations of flesh color. . . . . Now we will speak of coloring a dead man; that is to say, his face, his body, or any naked part that is visible. Use no rosy tints, because dead persons have no color; add a little light ocher; mark the outlines with sinopia, mixed with a little black, which is called sanguine. But, I warn you, use no color which you have not seen. . . . . When you have to paint the bones of Christians make them of the flesh color ('incarnazione'). Having to paint a wounded person, you must lay a tint of pure cinnabar wherever the blood is to appear; then glaze this and the drops of blood with fine lake, tempered in the usual manner."

In subsequent chapters he describes methods of taking a life-cast of the face, casts of the entire figure, of a man, a woman, or an animal; how to model from life, etc. And so through the whole gamut of that earlier practice among the Giottesschi. Parenthetically, I would say that all the great technical treatises on the science of perspective and the science of bodily proportion, except Dürer's, issued from Florence; Ghiberti, Alberti, Leonardo, Michaelangelo, Rosso de Rossi, and you may add the work of Luca Paciolo, "Divina Proportione." Likewise, in the great outpouring of mathematical works, between the years 1472 and 1500, Florence had her full share (213 mathematical treatises were printed in Italy in those 27 years, 100 of these issuing from press between 1480-1490). The Florentine artists were, many of them, gifted mathematicians; this is very true of Brunelleschi and his pupils. Vasari says of Verrocchio, "In his youth Andrea gave considerable attention to science, more especially to geometry; Ucello likewise." Of Piero della Francesca, Vasari says: "He understood all the most important properties of rectilinear bodies better than any other geometrician." The greatest mathematical compiler in all Italy, Luca Paciolo, received his training from this same Piero della Francesca, as well as later assistance from Leonardo. Da Vinci himself was a mathematical genius of the purest ray. "Let no man read me who is not a mathematician. No human investigation can lay claim to being true science unless it can stand the test of mathematical demonstration. The man who undervalues mathematics nourishes himself upon confusion. The more mathematics, the more science." A recurring phrase in Leonardo's manuscripts is "la somma certezza della matematica" (the apodictic certainty of mathematics). Certainly the science of quantity had gained a great exponent and protestant in Leonardo, for he was one who was ready to place his transcendent talent as an artist at the disposal of pure science—almost without reserve.

I have digressed so far only to prove again to you that we here are dealing with unabashed scientists of the brush. Leonardo is merely the end-result of uncompromising realism. His passion for research is the outcome of Verrocchio's method. His master, Verrocchio, was taught by Baldovinetti, who in turn derived from Domenico Veneziano, who in turn derived from Andrea del Castagno. The last named, by the way, is described by a contemporary as "amatore della difficulta del arte." There was no attempt at evasion of the natural difficulties encountered by the representative arts at Florence after the coming of this man Andrea "of the hung." The scientific impulse found firm lodgment in him. It seems as though the desire were stronger in him than in his fellows to rid himself at once of elusive and symbolic half-statement and gain a share ever greater in the divine truth of form. He is not one to be easily put off with the elementary rules of bodily proportion. He is bitten with *corporum intus curiositas*. He will take all the hazards of his art, will descend to the charnel-house under the New St. Mary's Hospital and make assay of the "Science of the Sepulchre." In fact, he painted a figure of Saint Andrea "beneath the charnel-house in the cemetery of Santa Maria Nuova," adds Vasari; and he gave such solidity of form to his patron saint that the superintendent of the hospital had him decorate a part of the principal chapel of the church above (1450-52). Domenico Veneziano and Baldovenetti, master and pupil, both first-rate exponents of the new art, were at work there—but I anticipate. Leonardo says that Andrea drew his muscles so boldly that his figures looked like bags of nuts. This was true of his drawings only, not of his painted subjects. Exaggerated myologic detail is constantly met with among the sketches of the early Florentine painters. So it is among their pen and chalk drawings that we find strongest evidence of a taste for anatomy among these masters. Certainly Castagno had such a taste to a degree! He did not rest content with his knowledge of the superficies of structure. The unclouded drawing of the human figure in action required that he plunge into the complexities of his subject. The muscles, situated immediately under the skin, interest him primarily, but the joints and the skeletal mechanism under the operation of the muscle-groups is a matter of profounder moment to him. Certain aspects of the artist's *problem of form* are speeding on the way to resolution, under guidance of this man. It looks as though Castagno is under some grim bond to naturalism, whereby he engages to trace no outline that does not express his personal knowledge. Whatsoever his minerver brush relates, he has experienced. The muscles which he draws so boldly are solidly attached to actual underlying skeletal parts, and are capable of putting solid members into action. Because he cannot render tip-toe movement and the grace of life, he fails to please Mr. Ruskin. Not that that matters in the least! The thing to note is Andrea's attempt at an utter fidelity to fact, his portraiture of the body as well as the face, his forthright draughtmanship—these he got through anatomies.

The manner in which he arrived at a point of serene power, and vastly influenced the Florentine school, is precisely the manner of Leonardo. "Study the science first; then follow the practice." Better teaching was at hand, perhaps, in Umbria; yet Tuscany proceeded to follow Castagno. He greatly influenced Pollaiuolo and Botticelli, and to a lesser degree Doménico Veneziano who, I repeat, taught Baldovinetti, who taught Verrocchio, who taught Leonardo. Thus the scientific mantle of Castagno passed down in a line of direct descent to Leonardo. These men no longer studied intensively the nudes of Masaccio in those epochal frescoes flanking the Brancacci Chapel at the Carmine—I mean "Peter Baptizing," or "The Expulsion from Eden"; nor did they travel to Castiglione d'Olona, to view Masolino's "Baptism of Christ." They still regarded these pictures as sheer miracles of unimagined power of drawing—precious testimony from the founders of an art that henceforth was to be grounded not upon intuition but upon perception and knowledge.

I pass to Paolo Ucello. In Paolo Ucello we have an awful example of an artist whose zeal for the house of science "hath eaten him up." He had an over-freight of scientific interest. Of adventurous mental temperament, he went squarely to the attack on all the laws governing perspective. As he died in 1475, three years before the first book on practical arithmetic was printed, we may conceive of his difficulties. He turned to Giovanni Manetti for help. "With this philosopher," says Vasari, "Paolo conferred very frequently, and held continual discourse concerning"—what? The formulæ of art? No, "the problems of Euclid." Donatello, pleading possibly in behalf of the neglected wife of the poor science-smitten man, once expostulated with him: "Ah, Paolo, with this perspective of thine, thou art leaving the substance for the shadow." For his own private worship Paolo painted five panels in his bedroom representing his five heroes—Giotto, in painting; Brunelleschi, in architecture; Donatello, for sculpture; himself, for perspective; Giovanni Manetti, for mathematics. Ucello is truly to be counted a hero—his self-sacrificing labors enabled later painters to proceed from a basis of exact science to the far nobler pursuit of ideal beauty. Those scores of unknown industrial craftsmen like him, in Florence, everywhere eager to impoverish themselves and render up their lives for science, were smoothing the way for our Leonardo.

Much could be said about Donatello. To him, as to any worker "in the round," bred to the processes of sculpture and metal-shaping, bone and brawn construction was the paramount interest. He modeled the body as he modeled the head, in a spirit of uncompromising portraiture—to embellish is to falsify. He expresses his hatred of simulacra, in every touch of chaser and chisel. To prove that Donatello assisted at anatomies, at least from the spectators' bench, we have the sole evidence of that Paduan bronze tablet representing "The Anatomy of the Miser's Heart" in the series of Miracles of St. Anthony. You will recall that the forceful rendering of the human form, the muscular litheness and movement of Donatello's work, were the especial admiration of Michaelangelo, and helped mightily to form him. Donatello's bronze "David" at the Bargello is the first nude statue of the Renaissance. (It is interesting to note that the first mention of practical school-anatomies at the Florentine University coincides with Donatello's birth-date.)

The "Statuta universitatis et studii Florentini," under the date 1387, deal with dissection of bodies at the school, under six explicit heads, specifying the duties of the Rector and Beadle; the delivery of the bodies by the officers of the Podesta; the crying of the anatomy through the school precincts; the choosing of a responsible student whose quarters are large enough to admit the chosen advanced students who pay and the few readers and the Rector who do not pay; the careful accounting of the costs—the fees to the porters and to those who say mass over the body and bury it, as well as the cost of the good cup of wine given to brace each quasi-valiant heart before and after the ordeal. "In case God grants the Studio to grow, then let the Podesta see to the delivery of not two but three bodies of alien criminals each year; whatever their foul felonies be, let them be hanged (not burned as the wont is with witches, nor beheaded) and delivered the same day, for corruption comes on apace."

Now, an artist rarely saw such a school-anatomy in Florence—the audience was strictly limited in ways that would naturally exclude him, unless he was a friend of the Rector. However, there were three ways open to him to appease his "corporum intus curiositas": (1) To wait for a public dissection (which occurred at unconscionably long intervals); (2) to go to private dissections performed by his doctor friends; (3) to make up little post-mortem parties of his own. Almost invariably would he choose the third method. He went or sent to the gibbet, or to the newly laid grave, if a poor man; if a man of repute and in good odor with the brethren, he could work in the dead-house of the hospitals. I dare say the large majority of Leonardo's dissections were done in the hospitals of Florence, Milan and Rome. Michaelangelo did his anatomies in lordly style—hired a house in the St. Agata quarter and got Realdus Columbus to ship him subjects. Michaelangelo's pupil and chief assistant in the Medicean tombs, Montorsoli (who probably did the figures of Cosimo and Damian flanking the monument), worked even more intimately with the anatomists of the schools (Genoa). There is rivalry, almost bitter at times, between the anatomizing artists and the practical anatomists. Hear what Condivi has to say here (Condivi is the pupil and biographer of Michaelangelo): "From a child Michaelangelo was a hard worker, and to the gifts of nature added study, not using the labor and industry of others, but desiring to learn from nature herself; he set her up before him as the true example. There is no animal whose anatomy he did not desire to study, *much more, that of man,* so that those who have spent all their lives in that science, and who make a profession of it, hardly know so much of it as he. I speak of such knowledge as is necessary to the arts of painting and sculpture, not of other minutiæ that anatomists observe." So much from the artistic camp—now the other side. Vesalius, on page 175 of the work on China root, says that he anatomized a Florentine patrician, Prosper Martellus; and then a few

pages later comes this very curious outburst: "As for those painters and sculptors who flocked around me at my dissections, I never allowed myself to get worked up about them to the point of feeling that I was less favored than these men, for all their superior airs."

Now to return to the age of Donatello. There is a largeness of intelligence, a quickness of sympathy, a god-like comprehension in men of the stamp of Ghiberti, Brunelleschi, Donatello, della Robbia. They were many-sided, like Orcagna, possessed of eight souls. The task of the revival called for men of amplest powers. Florence contributed men distinguished for their peculiarly versatile genius and the vast variety of their acquirements. The qualities of daring, variety, and brisk vigor are found (as in one of the pupils of Ucello and Donatello, Antonio Pollaiuolo) sometimes disassociated from breadth of vision, elevation of feeling, and human sympathy. However, as Pollaiuolo was "the first Florentine master to study human anatomy systematically in the modern sense," he will serve to bring our loose account to the tightening point. Born in 1429, the grandson of a poulterer, apprenticed to Ucello, then to Donatello, for whose cruel naturalism he had a keen taste, we see him moving along paths of detailed, severely accurate, technique, in niello engraving and in the casting of little bronzes. He is painter, sculptor and jeweler, too. Pretty soon we see him moving along paths more elaborate, of purely objective scientific inquiry, in quest of the intimacies of form; and, with a goldsmith's regard for detail and fineness of handling, he begins to anatomize. "He dissected many human bodies," says Vasari, "to study the anatomy, and was the first to investigate the action of the muscles in this manner, that he might afterwards give them their due place and effect in his works." Ruskin has something injurious to say anent this; I quote: "The virtual beginner of artistic anatomy in Italy was a man called the 'Poulterer,' from his grandfather's trade; Pollajuolo, a man of immense power, but on whom the curse of the Italian mind in this age was set at the deepest. . . . . Pollajuolo, Castagno, Mantegna, Leonardo da Vinci, Michaelangelo, polluted their work with the science of the sepulchre and degraded it with presumptuous and paltry technical skill. . . . . Foreshorten your Christ, and paint him, if you can, half putrefied—that is the scientific art of the Renaissance." This occurs in a note to "Ariadne Florentina." It is a note that does not require any answer. Some day let us learn to shun the frumious Ruskin as we would the bandersnatch; when he is raw and intemperate, and turbid and fallacious, at least then, let us not go down in craven submission to him. At any rate, Pollaiuolo got at the mainsprings of bodily movement; he recorded his progress in anatomy in a model series of drawings, full of "bizarre energy," which were eagerly passed around among the artists of his day. These drawings reveal him, as Berenson declares (in his "Drawings of the Florentine Painters"), as "one of the greatest masters of movement that there ever has been; one of the ablest interpreters of the human body as a vehicle of life-communicating energy and exulting power." His influence was as great as it was bracing. He even influenced Germany through Dürer. He affected all schools, all sorts and conditions of artists, Botticelli, Signorelli, Piero di Cosimo, possibly Verrocchio, to whom I now turn.

Like "the Poulterer," Andrea del Verrocchio was a pupil of Donatello. He was a goldsmith and general art entrepreneur, of highest renown as a teacher of painters. As a sculptor he was the best between Donatello and Michaelangelo. His great bronze equestrian statue of Colleone will mark him as "ganz apart." Like a colossus he bestrode the crews of bronze-casters in Florence, who were turning out objects for household decoration. He was the very man to inspire the young Leonardo, a supreme technicist, whose work was finished *ad unguem;* a scholar, whose devotion to the mathematical disciplines I have already dwelt upon. Vasari says that he possessed two drawings by Verrocchio "of two horses with various measurements, and the proportions according to which they are to be increased from a smaller to a larger size—all of which are correct and free from error." In the bronzes of Verrocchio, and in his panels and drawings, searching anatomic features can be noted; and if we can entirely trust Mathias Duval (who after all is an authority in the matter), there also exist certain *écorchés* or flayed figures, purely anatomical in purport, which were used in Verrocchio's "Bottega" and were fashioned by him for the use of his pupils.

There is no doubt as to anatomy being a part of the established curriculum in the Florentine "Bottega" from now on. Men from remote schools came to steep themselves again in Florentine science. Piero della Francesca, and his heavenly appointed spiritual heir, Signorelli, came from Umbria wanting to draw "the nude, the whole nude and nothing but the nude."

There are increasing evidences of an easy intimacy between artists and physicians. Both Piero della Francesca and Ridolfo Ghirlandaio painted a "donna gravida," and we have an occasional scene in a medical ward (Domenico di Bartolo at Sienna), an interior of a bathing establishment (Franciabiagio), a sepulchral monument (Verrocchio Tornabuoni). Bernado Rossellino did a fine portrait bust of the physician Giovanni da San Miniato, signed, and dated 1456 (now in South Kensington). Office consultations are found, in miniatures in manuscripts, and also the well-known low relief by Andrea Pisano on the campanile of Santa Maria del Fiore. It is called *Medicina* and is second in the series of mechanical arts. The scene is a consulting room; the surgeon is seated to the right, in a cathedra, urine-casting; next him, his assistant holding a mandrágora plant in his left hand and rudely pointing with his right thumb to a gravid young woman standing at his right. Near the door are two women carrying urinal baskets. Of course, out of flattery to Cosimo de Medici, Florence, from early days, was flooded with representations of the healing saints Cosimo and Damian, in fresco, oil and marble. Hygeia with her serpent is one of the archaic details of the Mandoria portal of the Duomo.

Everywhere throughout the city you will meet with hints of primitive, as well as present, correspondencies between the plastic arts and medicine. The painter Cosimo Roselli, founder

of a school marked by enthusiasm for studies anatomical, should never have become an artist at all, but a physician; for it was his family that had kept the immemorial "Lily Pharmacy" hard by New St. Mary's Hospital. How these easy intimacies arose between physic and the figurative arts would be hard to explain in any other way than the one I shall attempt to use, simple and obvious as it is! It was by the hazard of association in one and the same guild that the anatomists and artists of Florence made their magnetic contacts. The painters formed a sub-membrum of the "Guild of Physicians and Apothecaries." They all belonged—Giotto, Masaccio, Castagno, Ucello, Verrocchio—to the membrum pictorum of the Guild of Physicians and Apothecaries. Masaccio joined the guild first as an apothecary (in 1421, at the age of 19); then he matriculated under the membrum pictorum (in 1423). You see, the apothecaries included color handlers, the "spetiarii, qui emunt, vendunt et operant colores et alia ad membrum pictorum spectantia memoratum" (apothecaries who buy, sell and deal in colors and other materials needed by the artists). By virtue of this affiliation the artists and doctors were thrown together in all the multiform guild functions; they sat together in the guild Council; walked together under the same banner in pageants. It cannot, therefore, be a matter of surprise to learn that Giotto was the friend of Dino del Garbo and Torrigiana, or that Luca della Robbia (almost 200 years later) was a friend of the founder of pathological anatomy, Benivieni, although the latter was almost half a century (49 years) younger than Luca. (Luca, you will recall, executed the great arms of the Guild of Physicians, a polychrome terra cotta medallion in Or San Michele.)

The opening chapter of artistic anatomic studies at Florence, ending with an account of Verrocchio, furnishes us with only a meagre amount of good provable and usable material as compared with the account of Leonardo or Michaelangelo and their followers. With Leonardo, of course, would be grouped Lorenzo di Credi, de Predis, Boltraffio, Sodoma, and Melzi who was custodian of his scientific manuscripts. Michaelangelo's group would be large and difficult to handle, but we would single out Montorsoli and Sebastian del Piombo. There is yet another group to be studied, *i. e.*, Cosimo Roselli, Piero di Cosimo, his pupil, Andrea del Sarto, his pupil in turn, and the entire school of del Sarto (which was under the influence of Michaelangelo), Pontormo, Franciabiago, Rosso Fiorentino.

And to that immortal ruffian, Benvenuto Cellini, who in his diary sums up the tale by remarking that the "essential thing in art is thoroughly to understand how to paint the nude," to him separate space might be given, as to one, somewhat isolated from the moral order, but within the pale of an Art that was still in the midst of her traffickings with Science.

PROCEEDINGS OF SOCIETIES.

THE JOHNS HOPKINS HOSPITAL MEDICAL SOCIETY.

DECEMBER 6, 1915.

1. **Report of Cases: Chloroma and Acute Myeloid Leukemia.** Dr. John T. King, Jr.

   To appear in full in a later number of the Bulletin.

2. **Chemical Studies in Bichloride Poisoning.** Dr. D. S. Lewis and Dr. T. M. Rivers.

   To appear in full in a later issue of the Bulletin.

3. **Experiences with the Epidemic of Typhus Fever in Serbia.** Dr. R. P. Strong.

   To appear in full in a later issue of the Bulletin.

DECEMBER 20, 1915.

1. **Spontaneous and Experimental Leukemia of the Fowl.*** (Abstract.) Dr. H. C. Schmeisser.

In this paper was presented the first unit of an extensive study of a transmissible leukemia of the fowl.

Careful examination of a case of spontaneous leukemia of the fowl showed this to confirm in every detail the findings of Butterfield, Mohler, Warthin, Kon, Soshestrenski, Ellermann and Bang, Hirschfeld and Jacoby, and Burckhardt. The blood and anatomical findings closely simulated those of human myeloid leukemia.

By intravenous and intraperitoneal injection of an organic emulsion from this bird the disease was produced in 13 previously healthy fowls, and carried into the fifth generation, giving a blood and anatomical picture identical with that of the spontaneous case, and a clinical and anatomical complex which is analogous to that in the human disease. This is in confirmation of the work of Ellermann and Bang, who first successfully transmitted the leukemia of the fowl.

* Published in detail in the *Journal of Experimental Medicine*, 1915, XXII, 820-838, and fully illustrated in The Johns Hopkins Hospital Reports, 1916, XVII.

DISCUSSION.

Dr. W. A. Baetjer: I have been much interested in this same subject from the standpoint of human leukemia, where of course no one has been able to progress anything like so far as Dr. Schmeisser has been able to go in the study of fowls. It seems to me that Dr. Schmeisser has opened up the possibility of solving the problems of this disease, which may be of great aid in studying the leukemia of man. It comes at a most interesting time with relation to human leukemia in the sense that there have been, for the past two years particularly, an increasing number of reports which have seemed to suggest the possibility that at least some cases of human leukemia are of an infectious nature. I presume from what Dr. Schmeisser has said that he regards this disease of fowls as being infectious rather than neoplastic. Most of the evidence in the human cases, it seems to us at least, is distinctly in favor of the same conclusion. In that sense it throws a great deal of light on human leukemia and comes at a time when most of those who are working have held back from stating whether they think it is an infection or not. In the last two years, at least three cases have been reported in which an organism has been isolated from the spleen or glands of patients. But in no case have investigators been able to reproduce the disease with these organisms, nor has it been possible in any way to reproduce the typical disease, as Dr. Schmeisser and others have done with the fowl. It seems to me, then, that this offers a very definite advance from the standpoint of human leukemia, in that

you can get in the fowl a picture that is clinically and pathologically similar to that of human leukemia, but which differs from it, in that it has been transmitted from animal to animal by means of tissue innoculations. No one can doubt, I think, that it corresponds in most ways to human leukemia. That it is the same disease, I think is still somewhat open to doubt, because human leukemia presents at the present time a very mixed picture. There are cases which correspond with the cases Dr. Schmeisser has reported in the fowl. Others do not, from the standpoint of blood examination, from the clinical course or from pathological findings. Dr. Schmeisser has brought out that you can have an aleukemic stage coming on in the same series of animals. In aleukemia the picture is interesting at the present time in connection with the efforts on the part of many to put such diseases as Hodgkin's disease and certain other obscure glandular enlargements into the class of leukemia.

The most interesting feature of the paper is the evidence it offers that a disease which simulates certain types of leukemia in the human being occurs in the fowl, and that in the fowl the disease is definitely infectious. This point gives us far more to go on than we have ever had in dealing with human leukemia in regard to its etiology.

I should like to ask Dr. Schmeisser with regard to the pathology of leukemia, whether he feels that it is a very distinct and specific type of change; *i. e.*, whether the pathology of leukemia in general, acute and chronic, is specific. I should like to ask also whether, from what he has seen in his slides, he could offer any conclusive evidence on a point which has caused a good deal of confusion in human pathology, namely, whether the enlargement of the spleen in human leukemia is the result of an infiltration with the same cells which circulate in the blood, or whether it is due to what is termed a myeloid metaplasia?

DR. S. R. MILLER: I should like to know Dr. Schmeisser's basis for calling structures blood platelets which are larger in size than red blood cells and which contain protoplasm and dividing nuclei.

DR. SCHMEISSER: In answer to Dr. Baetjer's questions: (1) Typical cases of both lymphoid and myeloid leukemia produce pathological pictures which are specific. (2) Whether the presence of the myeloid cells in organs and tissues outside of the bone marrow in leukemia is due to an infiltration and proliferation of the same or due to their origin *in loco* by a reversion of the vascular endothelium to its embryonic hematopoietic function and then a proliferation, I am not at the present time able to offer any conclusive evidence.

In answer to Dr. Miller's question: The structures referred to were called blood platelets simply as a means of classification and not because they are known to be analogous to the human blood platelet.

2. The Quantitative Study of Analgesia after Opium Alkaloids. DRS D. I. MACHT, N. B. HERMAN and C. S. LEVY.

A preliminary communication on the subject was published in the proceedings of the *National Academy of Sciences* for December, 1915; and the complete paper appears in the *Journal of Pharmacology and Experimental Therapeutics* for January, 1916.

3. The Action of Papaverin on the Ureter. (Abstract.) DRS. D. I. MACHT and J. T. GERAGHTY.

DR. MACHT: In connection with the preceding paper I wish to report a few experiments performed by me within the last few weeks, in order that the same may be recorded in the minutes of this meeting. I have already called attention to the general analgesia produced to some extent by injections of papaverin. This rare alkaloid of opium possesses a number of very interesting properties. I have already shown in the *Journal of Pharmacology and Experimental Therapeutics* for September, 1915, its action upon the respiratory center, and have also called attention to its stimulating effect upon the heart and to its interesting action on the coronary circulation in the *Journal of the American Medical Association*, May 1, 1915.

Perhaps the most interesting property of this drug is the one to which attention has been called by Pal of Vienna and his coworkers Popper and Franckel. These authors point out that papaverin lowers the tonus of all organs composed of smooth muscle. I have been able to confirm their results by personal observations on isolated arteries, uterine strips and intestinal muscle.

Very recently I have had the opportunity of studying, together with Dr. Geraghty, a patient suffering from ureteral calculus, and the idea occurred to us to introduce a solution of papaverin through the catheter directly into the ureter, just below the stone. Knowing from my experiments that papaverin is not toxic, there was no objection to this procedure. Five cubic centimeters of a 2-per-cent solution of papaverin hydrochloride were therefore introduced. On the same afternoon the patient experienced slight colicky pains, and on subsequent examination the stone was found to have descended a considerable distance. After a second treatment with papaverin the patient spontaneously passed the calculus. Dr. Geraghty, while making the injection, could observe with his eye through the cystoscope that the ureteral orifice dilated after the introduction of the drug.

Following this experience I have performed a number of experiments on the isolated ureter of a pig. I was surprised to find that the ring of pig's ureter suspended in oxygenated Locke solution began to contract rhythmically not unlike a frog's heart. These contractions continued spontaneously for several hours at a time. On addition of 5 mg. of papaverin, and even less, there was an immediate relaxation of the ring and a discontinuance of the contractions. Exactly the same observations were made on the human ureter obtained from an operation. So far as I know, these observations have never been made before. It may be further stated that papaverin not only exercises a general analgesic effect, but also acts as a local anesthetic. Attention has been directed to this fact by Pal, and it has been confirmed by me from observations on myself and some of my colleagues.

In this connection I may as well add that even more interesting than the action of papaverin upon the ureter has been found by me to be the effect of epinephrin. The smallest amounts of this drug, I was surprised to find, increased the frequency of the ureteral contractions and markedly increased the tonus of the ureter. Indeed, even a *quiescent* ureter, that is, a ring which is almost dead and is not contracting, can be revived by the addition of a little epinephrin. A general study of the pharmacology of the ureter is now being carried on.

DISCUSSION.

DR. GERAGHTY: Theoretically, a stone which is sufficiently small to leave the pelvis of the kidney and pass into the ureter should be spontaneously expelled, because the narrowest part of the ureter is its junction with the pelvis. It has been estimated, however, that only about 75 per cent of the stones which enter the ureter are spontaneously expelled. Many of those which become impacted can be removed by manipulative procedures. About 15 or 25 per cent of the stones become arrested in the vesical portion of the ureter and practically all of these stones can be readily removed by intravesical procedures. For stones that become impacted above this point we have heretofore relied upon the passage of an ureteral catheter, with the idea of changing the position of the stone, and possibly its axis, in order to allow its shortest diameter to lie in the ureteral canal. Frequently stones are passed which have been manipulated by the ureteral catheter, even those that have been present for several years. The use of the thermocatheter seems to promise more than the use of the simple catheter, because by means of the heating we not only can

manipulate the stone, but also can secure a certain degree of dilatation.

A few months ago I had a most striking example. The patient had a stone impacted at a point 12 cm. from the vesical orifice. A special thermo-catheter was passed as far as the stone, heated to about 115° and then slowly withdrawn. When the catheter was removed from the ureter the stone was found in the vesical portion, having followed the catheter directly down.

Papaverin seems to promise considerable help, because it produces marked relaxation, but some technique will have to be devised by which we can limit the action of the papaverin to the ureter below the stone, or else secure dilatation without paralyzing completely the ureter wall. In order to expel the stone it is necessary to preserve the propelling properties of the ureteral wall itself.

In the case reported by Dr. Macht the stone was very promptly expelled following the injection, without any colic.

JOHNS HOPKINS HOSPITAL HISTORICAL CLUB.

DECEMBER 13, 1915.

1. The Rôle of Certain Florentines in the History of Anatomy, Artistic and Practical. DR. EDWARD C. STREETER.

This paper appears in full in the current issue of the BULLETIN.

DR. H. M. HURD: Just twenty-five years ago The Johns Hopkins Hospital Historical Club was organized. I suppose I am the only one here to-night who was present at its first meeting, at which Dr. Welch presided and Dr. Osler gave the first paper. Dr. Osler was very much chagrined at the form of the printed programme. In addition to reading his paper, he had intended to have an exhibition of old books, and had numbered his talks *1* and *2*. One of the books he had planned to show was John Morgan's Treatise on Education, which he had numbered *1*. Unfortunately, through an error of the printers, the programme read: "I John Morgan. By William Osler." Dr. Welch also gave a talk at that meeting, I think on books. There were not a great many present, and I am afraid we all felt the society was not destined to be very long lived. It was supposed that we should soon see all the old books we could obtain, and that then interest would naturally die out.

It is astonishing, however, how interest in the meetings of the club has been kept up and how much good work has been done. We have had papers of course not displaying any great amount of scholarship, but these have been of a character to indicate where sources of knowledge could be traced. The several papers which Dr. Billings gave us were always extremely valuable. He at one time delivered here, over a period of three or four years, a series of lectures on the history of medicine and used to appear at the meetings and discuss the questions presented. A great many of Dr. Osler's brilliant essays were read before this Historical Club, and also many of Dr. Kelly's. We have had unusual kindness shown us by scholars throughout the country, indeed I may say throughout the world. We have had papers from men whose writings and whose presence have been a great inspiration to us.

We have now begun the second quarter century of our existence. As I have said, I was present at the first meeting, and I am hopeful that I shall be present at the celebration of the second quarter century. If I cannot be, it is my wish that the society will still be in active operation.

NOTES ON NEW BOOKS.

Mechano-Therapeutics in General Practice. By G. DE SIVIETOCHOWSKI, M. D., M. R. C. S. Cloth, $1.50. (*New York: Paul B. Hoeber, 1914.*)

The author of this volume has succeeded in emphasizing the simplicity of the technic of massage. The subject is treated with unusual clearness and is singularly free from complicated and confusing description of a technic that depends upon its very simplicity for its effect.

In the chapter on "indication and contra-indication," we notice with pleasure the absence of the enthusiast. We may not agree with the author on his treatment of fractures, nor will results obtained in individual cases always be convincing; still the work should be useful to those interested in the subject. C. R. A.

Diseases of the Nose, Throat and Ear—Medical and Surgical. By WILLIAM LINCOLN BALLENGER, M. D. Fourth Edition. (*Philadelphia and New York: Lea & Febiger, 1914.*)

From certain points of view this is perhaps the most valuable book on this subject in the English language. On the other hand, even the thoroughly revised fourth edition is open to criticism.

Although containing over one thousand pages the book makes only a very feeble attempt to correlate the focal lesions of the nose, throat, sinuses, larynx and ears, as seen by the laryngologist and otologist, with the general manifestations as seen by the internist, the pediatrician and the neurologist.

The chapters dealing with the inner ear are the least satisfactory in this edition. To those not already familiar with the fundamental principles, the author's description would be extremely confusing, since he has not clearly distinguished between facts and hypotheses. This criticism applies particularly to the vestibular apparatus; the chapter on the functional tests of hearing is well worth reading.

The book, as it stands, will prove of great value to the specialist, but the author could have made it of equal value to the medical student and general practitioner, by emphasizing the relation of focal infections to the various general systemic disorders.

S. J. C.

Cane Sugar and Heart Disease. By ARTHUR GOULSTON, M. A., M. D. $2.00 net. (*New York: Paul B. Hoeber, 1915.*)

It is to be regretted that this book should have been published before the glaring mistakes could have been called to the attention of Dr. Goulston; but then, one wonders, Would there have been anything left to publish?

The book of one hundred pages starts with sentences culled from the work of cardiac authorities that serve as a frame-work for twenty pages of highly improbable and unsatisfactory suggestions that are "conclusive" as to the value of cane sugar as a cure-all in cardiac therapy.

This conclusion is suggested by about fifty pages of cases that rival the columns in the daily papers devoted to the testimonials of patients that have been cured by twenty drops of Duffy's Malt Whisky—both in the medical knowledge portrayed, and in the satisfactory results. One is somewhat abashed at the ease with which the writer diagnoses a case of "auricular fibrillation with auricular tachycardia" by the radial tracing, and must, perforce, marvel with the author at the complete recovery by the use of cane sugar in this and other cases. But when the reader notes the development of soft vessel walls in a case of atheromatous arteries, following a year of five ounces of cane sugar daily, and the cure of a case of tuberculosis by the same therapy, he will probably smile as he closes the book. The remaining twenty pages that he misses thereby are made up of radial tracings wrongly diagnosed, an abstract from the Times on The Strain of Life in the Great Cities, and a warning that the use of beet sugar is without avail.

E. W. B.

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